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THE PSYCHOLOGICAL REVIEW

STIMULUS INTENSITY DYNAMISM (*V*) AND STIMULUS GENERALIZATION

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INTRODUCTION

There seems little doubt that a general dynamic molar law based on stimulus intensity (*i*) exists and functions on a large scale. We propose to call this principle *stimulus intensity dynamism* (*V*). The concrete manifestations of this law have frequently been observed by experimentalists, especially in connection with the phenomenon of stimulus generalization, which explains the conjunction of the two principles in the title of the present article. We shall begin by examining some of the concrete phenomena so far reported, after which we shall attempt to arrive at a more precise though provisional formulation of the law and its joint action with stimulus intensity generalization in the determination of reaction potential magnitude.

SOME EXAMPLES OF STIMULUS INTENSITY DYNAMISM

We turn first to a stimulus-intensity generalization study carried out by Hovland (6) on human subjects. When the generalization effects of four sound intensities employed were presumably equalized, the amplitudes of

galvanic skin reactions averaged 10.1 for a stimulus of 40 decibels and 16.55 for a stimulus of 86 decibels, the number of reinforcements and everything else being constant. This greater response to a more intense stimulus is believed to involve stimulus-intensity dynamism.

J. S. Brown (1) tested the adient pull of rats to go to food which was associated with screens illuminated to the extent of .02, 5.0, and 5000.0 apparent foot candles. His report shows that when the sensory generalization effects were presumably equalized the pulling responses of his animals were 61.5 grams for .02 apparent foot candles, and 70.5 grams for 5000 foot candles. This greater response to a more intense stimulus is a clear case of stimulus-intensity dynamism. Incidentally it serves as an operational definition of the concept with which we are here primarily concerned.

Spence carried out a study (8) in which rats were trained to discriminate black from white, the two stimuli being presented simultaneously. He found that, other things equal, the rats trained to go to white made only 6.5 errors by going instead to black, whereas those trained to go to black made 19.3 errors by going instead to

white. This is a third case of stimulus-intensity dynamism.

At about the same time, Ruth Hays carried out in the writer's laboratory an unpublished study in which 20 rats were trained to jump against a black card for food and 20 others were trained to jump against a white card for equivalent food, the cards in this case always being presented singly. Equalizing other factors in the situation, the latencies of the jumps to white averaged 1.70 seconds, whereas those to black averaged 7.54 seconds. Since white is a stronger light stimulus than black and a reaction latency of 1.70 corresponds to a greater reaction potential than does 7.54, this is a fourth case of stimulus-intensity dynamism.

In 1886 James McKeen Cattell (2) carried out a meticulous study on two human subjects in which six different light intensities were used as the signal in a reaction-time experiment in which the hand was lifted. The means of the two subjects showed a consistent decrease in latency as the stimulus increased in intensity, the extreme mean latencies being .280 and .169 seconds, where the corresponding light intensity extremes were in the ratio of 1 to 1,000. This is accordingly regarded as a fifth case of stimulus-intensity dynamism.

More recently, Piéron (10) has reported analogous experiments with human subjects involving the effect of intensity of stimulations on reaction time in the cases of both hearing and taste. Both functions were found to be strikingly uniform and in agreement in showing that the stronger the stimulus, the shorter is the reaction latency and so the greater the reaction potential, quite as Cattell found.¹

¹ The two Piéron studies and that of Cattell are cited by Rashevsky (11) and by Householder and Landahl (5), where they are given a mathematico-neurological interpretation.

Finally, Hilgard and Marquis (4, p. 141 ff.) mention some conditioned-reflex studies which "demonstrate that response strength is a function of stimulus intensity, . . ."

In summary of the above facts it may be said that seven experiments appear to have uniformly found a definite tendency, other things approximately equalized, for trained responses to have a greater reaction potential (sE_R) as the stimulus increases in intensity whether tested by reaction latency, amplitude, or intensity. Accordingly the principle may be considered as qualitatively well established.

There remains the problem of stating the specific quantitative law of the functional relationship. Unfortunately the evidence on this more precise but scientifically necessary aspect of the question is far less satisfactory. For example, ideal evidence on this point might be furnished by an experiment like that of Hays, described above, if a number of other squads of animals were trained, each on a different known shade of gray as well as on black and white. The asymptotes of the several learning curves as measured in sE_R 's could then be expressed in a fitted equation stating the functional relationship of the several asymptotes to the associated light intensities. This would be the law of $sE_R = V = f(i)$. In the absence of such direct evidence we must make the best shift we can with the indirect evidence at present available.

The first data bearing indirectly on the quantitative functional relationship, $V = f(i)$, are those yielded by Brown's study just mentioned. In addition to the two means secured by averaging the opposite ends of his two stimulus-generalization gradients, there is also the average of the two

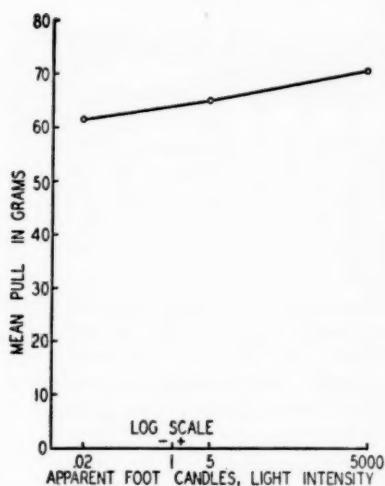


FIG. 1. Mean strength of conditioned adient pull of rats to food associated with lights of different intensity, generalization effects presumably equalized. Plotted on a logarithmic scale from values secured from Brown's data (1). Note that the distance on this scale from .02 to 5.0 is appreciably less than that from 5.0 to 5000.0.

approximate middles. All three values are shown graphically in Fig. 1. There it will be noticed that the relationship is nearly linear. In this connection it must be pointed out that stimulus generalization gradients of reaction potential are in general concave upward (7) and that averaging the middles of two such gradients will also yield a curve that is lower at its middle as related to the two ends than would be true for the undistorted relationship of stimulus-intensity dynamism to sE_R . This means that if Fig. 1 were corrected for this factor, its nearly linear shape would very probably become one of at least an appreciable convexity upward. It may be added that while strength of pull clearly is not sE_R , the two phenomena are evidently increasing functions of each other and the relationship

is presumably fairly linear throughout the main part of their ranges.

THE QUANTITATIVE LAW OF STIMULUS INTENSITY DYNAMISM (V) AS A FUNCTION OF STIMULUS INTENSITY (i)

Our second indirect evidence bearing on the quantitative functional relationship of $sE_R = V = f(i)$ is somewhat more specific, though again there is some uncertainty. As already suggested, Cattell's study is believed to give positive qualitative evidence on stimulus-intensity dynamism. Here it must be noted that six intensities of light stimulus yielded six mean reaction latencies from each of two subjects. Table I shows the light intensities and the corresponding mean latencies of the two subjects on a hand-lifting response, as calculated in the writer's laboratory. The functional relationship of the two variables was found to be almost perfectly fitted by the equation,

$$s^t_R = .113 \times 10^{-.59 \log i} + .167. \quad (1)$$

This is presented graphically in Fig. 2.

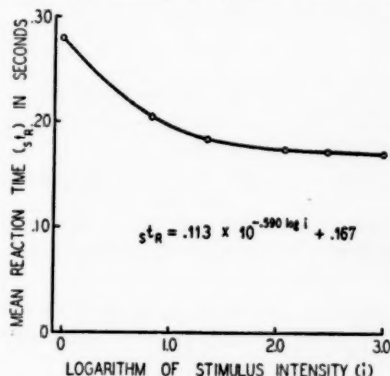


FIG. 2. Curve based on the means of the mean reaction latencies of two human subjects to light stimuli of different intensities. The original data were published by Cattell (2). The equation was fitted by H. C. Wilcoxon, the mean square deviation of the fit being .001.

TABLE I

Cattell's reaction-time results to various light intensities (2) with theoretically equivalent reaction potentials as calculated by us.

Light stimulus intensities (<i>i</i>)	1	7	23	123	315	1000
Mean reaction latencies (<i>st_R</i>)	.280"	.205"	.184"	.174"	.170"	.169"
Theoretically equivalent reaction potentials (<i>sE_R</i> = <i>V</i>)	.89	1.14	1.22	1.28	1.29	1.30

In our proposed interpretation of this law it is tentatively assumed that the reaction potential involved is essentially that of stimulus-intensity dynamism. Now, the incidental mediating computations are such that this equation will need to be used with an equation involving rat reaction latency, which is much slower. For that reason the equations of different origin must be adjusted so far as possible to what they would be if they were from the same origin. We have found that the medians of our rat latencies are roughly 13.6 times as long as Cattell's mean human reactions. We accordingly multiply the .113 and the .167 of equation 1 by 13.6, which yields the following equation:

$$st_R = 1.54 \times 10^{-.59 \log i} + 2.27. \quad (2)$$

We next note the following equation, which was fitted to some of our own empirical (rat) data (3):

$$sE_R = 2.845(st_R)^{-.483} - .599. \quad (3)$$

Now, substituting in equation 3 the values of *st_R* as it stands in equation 2, we have²

$$sE_R = V = 2.845(1.54 \times 10^{-.59 \log i} + 2.27)^{-.483} - .599. \quad (4)$$

² *V* is an expository device employed to indicate a theoretical reaction potential (*sE_R*) in which the quantitative influence of stimulus-intensity dynamism is explicitly included. It will serve also to indicate this particular factor which contributes to the determination of reaction potential value much as do *sH_R* and

Thus we have in (4) an equation which purports to express *sE_R* in terms of *i*, which we have been seeking. For example, substituting the value of *i* = 7 from Table 1 into equation 4, we have

$$\begin{aligned} V &= 2.845(1.54 \times 10^{-.59 \log 7} + 2.27)^{-.483} - .599 \\ &= 2.845(1.54 \times 10^{-.59 \times .8451} + 2.27)^{-.483} - .599 \\ &= 2.845 \left(1.54 \times \frac{1}{3.152} + 2.27 \right)^{-.483} - .599 \\ &= 2.845(2.758)^{-.483} - .599. \end{aligned}$$

$$\therefore V = 1.14.$$

Performing the corresponding substitutions and computations for the remaining stimulus-intensity values, we have the bottom row of values given in Table 1.

The values in question are shown graphically in Fig. 3. There may be seen what purports to be a representation of the functional relationship of *sE_R*, i.e., *V*, to *i*. In confirmation of our surmise based on the consideration of Brown's graph, a clearly convex-upward curve is obtained. This will be used tentatively until a more direct and dependable determination becomes available.

V. Subsequent workers will, of course, need to modify greatly the crude pioneering empirical evidence and equations here available.

Generalizing from the above considerations, we arrive at our primary principle or postulate of stimulus-intensity dynamism:

Other things constant, the magnitude of the reaction potential (sE_R , i.e., V) has an increasing monotonic relationship to the intensity (i) of the stimulus in question, the increase taking place at a progressively slower rate according to the equation

$$sE_R = V = A(1 - 10^{-b \log i}). \quad (5)$$

STIMULUS-INTENSITY DYNAMISM (V) AND STIMULUS-INTENSITY GENERALIZATION (sE_R)

So far as we can now see, equations 4 or 5 and Fig. 3 represent a first approximation to the molar law of stimulus-intensity dynamism in a form relatively uncomplicated by other prin-

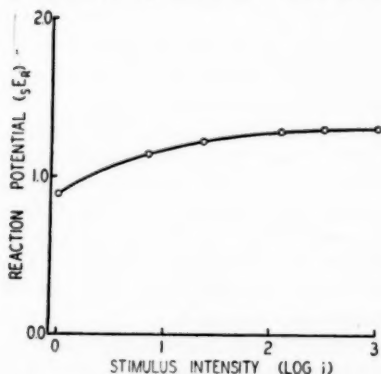


FIG. 3. Theoretical reaction potential plotted as a function of stimulus intensity on the assumption that Cattell's reaction latencies (2) represent approximately the dynamics of stimulus intensity ($sE_R = V$). Apparently the unit of stimulus intensity used by Cattell was 12.5 times as great as the subjects' stimulus threshold. Using the revised unit the curve falls to zero at a stimulus of 1 and the data are almost exactly fitted by the equation

$$sE_R = V = 1.328(1 - 10^{-.440 \log i}).$$

The fit was made by J. A. Antoinetti.

ciples. On the other hand, the results of Hovland (6), Brown (1), and Spence (8) cited above appear with equal clarity to be produced by the joint action of stimulus-intensity dynamism and stimulus generalization (sE_R). This necessarily raises the question of how these two molar laws interact and the characteristic results of this interaction.

Let it be supposed that a stimulus which is subject to variability in intensity (i), such as that used by Cattell (2), is conditioned to a response at the weak extreme of its log intensity (intensity 1 = log 00, rows I and II of Table 2), with one group of subjects, and at the opposite or strong extreme of its log intensity (intensity 1000 = log 3.0) with a second group. Then let it be assumed that the stimuli are tested for generalization at the middle point (log i = 1.5, row V, Table 2), and at the opposite extreme of stimulus intensity with each group of subjects. We assume as usual (7, pp. 186, 200) that the generalization gradient values are yielded by the equation,

$$sE_R = sE_R \times 10^{-kd}, \quad (6)$$

where sE_R is .89 at the weak intensity extreme and 1.31 at the strong intensity extreme (row III, Table 2), k is .15, and d is the difference between the log i of the point conditioned and of the point tested for generalization. In each case it follows that d is .00 at the point conditioned and 1.5 and 3.0 at the remaining two points. Substituting these three d values in equation 6 with sE_R at .89, we secure the stimulus generalization values appearing in row VI. Doing the same with sE_R at 1.31 we secure the stimulus generalization values appearing in row VII.

At this point we encounter the question of how stimulus-intensity dyna-

TABLE 2

The various steps in the computations leading to the two generalization gradients as influenced by stimulus-intensity dynamism, rows VIII and IX respectively.

I Stimulus intensity (i)	1	31.62	1000
II Log i	.00	1.5	3.0
III Equivalent reaction potential $sE_R = V$.892	1.24	1.31
IV V ratio (V) based on log .00	1.000	1.389	1.463
V V ratio (V) based on log 3.0	.684	.949	1.000
VI Simple generalization gradient starting at log $i = .00$.89	.530	.316
VII Simple generalization gradient starting at log $i = 3.0$.465	.780	1.31
VIII Row IV \times row VI $sE_R = sE_R \times V$.89	.736	.462
IX Row V \times row VII $sE_R = sE_R \times V$.318	.74	1.31

mism combines with stimulus generalization on the same $S \rightarrow R$ connection. In order to expose the problem to investigation in a definite manner we propose as a first approximation to the primary molar law involved the following postulate:

When stimulus-intensity generalization and stimulus-intensity dynamism are jointly acting on the results of the same learning, they combine by the multiplication of the stimulus generalization value (sE_R) by the ratio (V) of the corresponding stimulus-intensity dynamism (V) divided by the dynamism at the origin of the stimulus generalization, i.e.,

$$sE_R = sE_R \times V. \quad (7)$$

The stimulus-intensity ratios (V) based on the origins of the respective stimulus generalizations are presented in rows IV and V of Table 2. Multiplying each of the values of row IV by

the corresponding generalization value of row VI, according to equation 7, we have the joint values given in row VIII. Similarly, multiplying the V values of row V by the generalization values in row VII, we have the joint values given in row IX. The values of rows VIII and IX are the joint results we have been seeking. The values in question are represented graphically in Fig. 4.

Generalizing on the results particularly as shown in rows VIII and IX of Table 2, and relevant considerations, we arrive immediately at five corollaries:

The middle of a gradient which is convex upward must be greater than the mean of its two ends. Therefore, by row VIII,

$$.736 > \frac{.89 + .462}{2} \\ \therefore .736 > .676.$$

Accordingly:

I. When a response is conditioned to a weak stimulus and generalizes toward a fairly strong extreme of the stimulus-intensity continuum, the resulting effective gradient is convex upward.

The middle of a gradient which is concave upward must be less than the mean of its two ends. Therefore, by row IX,

$$.74 < \frac{.318 + 1.31}{2}.$$

$$\therefore .74 < .814.$$

Accordingly:

II. When a response is conditioned to the strong extreme of the above stimulus-intensity continuum and generalizes toward the weak extreme, the resulting effective gradient is concave upward.

By rows VIII and IX,

$$1.31 > .89.$$

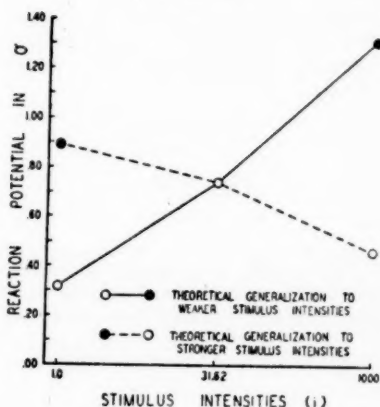


FIG. 4. Graphic representation of theoretical stimulus-intensity generalization as modified by concurrent stimulus-intensity dynamism when extending from weak toward strong stimulus intensities and from strong toward weak intensities. Solid circles represent the origins of the respective gradients.

Accordingly:

III. The origin of the gradient originating at the stronger stimulus extreme will have a stronger reaction potential (sE_R) than that originating at the weaker stimulus extreme.

Other things equal, the more steeply a gradient slopes downward from its origin, the greater will be the quotient obtained by dividing the value of its origin by that of its termination. Therefore, by rows VIII and IX,

$$\frac{.89}{.462} < \frac{1.31}{.318}.$$

$$\therefore 1.93 < 4.12.$$

Accordingly:

IV. The general trend of the joint sE_R gradient originating at a weak stimulus intensity and generalizing toward stronger stimulus intensities has a smaller tendency to a downward slope than that extending in the opposite direction between the same stimulus intensities.

By a simple inspection of Fig. 5 it follows that,

V. The two gradients intersect at their midpoint.

EMPIRICAL EVIDENCE CONCERNING THE SOUNDNESS OF THE THEORETICAL COROLLARIES

We are fortunate in finding empirical evidence bearing directly on all five of our corollaries. This consists of the quantitative studies of Brown (1) and Hovland (6). Because the Cattell study, which was used to derive our equation tentatively expressing the principle of stimulus-intensity dynamism, involved motor behavior, it is possible that the corollaries based on it may in some respects be compared more appropriately with the re-

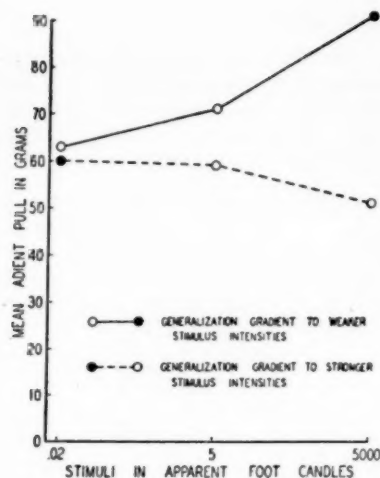


FIG. 5. Graphic representation of empirical stimulus-intensity generalization as modified by stimulus-intensity dynamism plotted to correspond roughly to Fig. 4. (Plotted from data published by Brown, 1.)

sults from Brown's study, which concerns motor reactions, than with those from Hovland's study, which concerns primarily glandular reactions. Moreover, the Cattell and Brown studies both employed light stimuli of somewhat similar magnitude. Nevertheless the results of both studies, so far as they have a bearing on our corollaries, are given—Brown's results in

Fig. 5 and Hovland's results in Table 3.

We proceed now to evaluate the two sets of empirical data by the same computational method utilized in deriving our corollaries from the numerical values contained in Table 2. The Brown values may be recognized on the graphs of Fig. 5; the parallel Hovland values have been taken from Table 3.

Brown's data present the following:

$$.59 > \frac{.60 + .51}{2}$$

$$\therefore .59 > .555.$$

Also Hovland's data present:

$$\frac{12.98 + 14.30}{2} > \frac{11.30 + 15.75}{2}$$

$$\therefore 13.64 > 13.53.$$

Both of the above inequalities show the respective gradients to be convex upward. It follows that, within the limits of the probable errors involved, Corollary I has empirical verification from two independent investigations.

Again, Brown's data present the following:

$$.71 < \frac{.60 + .91}{2}$$

$$\therefore .71 < .755.$$

TABLE 3

Empirical mean generalization response amplitudes to stimuli which are the same as or weaker than the intensity of the stimulus to which the response was originally conditioned, and the corresponding values of the gradient extending in the opposite direction. The values in bold-face type are the mean amplitudes of the responses to the stimuli originally conditioned. (Compiled from Hovland's published data, 6.)

Stimulus intensities in decibels	40	60	74	86
Mean response amplitudes based on conditioning to a tone of 40 decibels' intensity	11.30	12.98	14.30	15.75
Mean response amplitudes based on conditioning to a tone of 86 decibels' intensity	8.90	10.94	13.37	17.35

Also, Hovland's data present:

$$\frac{10.94 + 13.37}{2} < \frac{8.90 + 17.35}{2}$$

$$\therefore 12.16 < 13.13.$$

Both of the above inequalities show the respective gradients to be concave upward. It follows that within the limits of the probable errors involved, Corollary II has empirical substantiation from two independent investigations.

Once more, Brown's data present the following:

$$.91 > .60.$$

Also, Hovland's data present:

$$17.35 > 11.30.$$

The above inequalities show Corollary III to be verified, within the limits of the probable errors involved, by two independent investigations.

Further, Brown's data present the following relationship:

$$\frac{.60}{.51} < \frac{.90}{.63}$$

$$\therefore 1.18 < 1.43.$$

Also, Hovland's data present:

$$\frac{11.30}{15.75} < \frac{17.35}{8.90}$$

$$\therefore .72 < 1.95.$$

The Brown data above present a conventional verification of the less slope shown by the inequality from which the corollary was derived, as may be seen by a simple inspection of Figs. 4 and 5. The Hovland data, while conforming technically to the formula, really show a marked deviation from Fig. 4 in that the gradient, supposed theoretically to slope less downward, as a matter of fact slopes upward, as shown by the decimal value of .72. Actually this probably is a verification

of the logic of the basic theory, which pits stimulus generalization and stimulus-intensity dynamism against each other so far as the slope of the gradient extending from a weak stimulus intensity to a strong one is concerned. If the stimulus-intensity dynamism is very strong it will naturally reverse the downward slope which is natural to stimulus generalization operating by itself.

Finally there comes the matter of the intersection of the two gradients. Brown's gradients, as clearly shown by Fig. 5, do not intersect. On the other hand, a plot of the two Hovland gradients shows that they intersect about midway between the stimulus intensities, 74 and 86 decibels. Thus, ignoring the precise point of the intersection, one investigation substantiates Corollary V and one refutes it. Very likely the disagreement between the two empirical studies under consideration in this respect, and the failure of the corollary really to agree with both of them, depends upon the differing magnitudes in the two situations of one or more constants as yet not clearly identified.

The empirical verification of four out of five theoretical corollaries, together with a partial verification of the fifth, suggests that the postulates as a whole, including the multiplicative method of combining the sE_R and V involved, possess a considerable degree of truth but that some of them are clearly defective. As usual the nature of the defect must be sought in further research, both theoretical and empirical but primarily theoretical. As already suggested the remedy may lie in the exact determination of the magnitudes of the several constants involved, since the theoretical outcome is definitely sensitive to the variation of these magnitudes.

SUMMARY

Several experimental situations have been found which seem to indicate rather clearly that reaction potential is in part a function of the stimulus intensity involved. Because no specific investigations have as yet been made of this presumptive primary molar law, its mathematical statement can at present be given only tentatively. This is that stimulus-intensity dynamism is a monotonic function, increasing at a decreasing rate, of the logarithm of stimulus intensity.

An important situation in which stimulus-intensity dynamism influences reaction potential is found where stimulus intensity is the basis of stimulus generalization. This raises the question of how the two primary molar laws combine to produce the joint effect as a secondary molar law. To secure the first approximation here utilized, we reduce stimulus-intensity dynamism to a percentage ratio based on the point of origin of the generalization gradient, and then combine the two gradients by multiplication. This procedure, together with related postulates, leads to the deduction of several quantitative corollaries, four of which agree fully with empirical fact and one of which agrees partially. The conclusion is that the theory as a whole approaches closely to a true statement of the principles involved, but is clearly lacking in some respect.

An analogous situation of considerable theoretical importance as well as complexity is found where generalization occurs on the basis of perseverative stimulus traces. This matter requires separate treatment.

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BEHAVIOR OSCILLATION AND THE GROWTH OF PREFERENCE

BY J. C. TAYLOR

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When an animal is placed in an experimental situation where there are two paths to the goal, one longer than the other, or one involving a longer and the other a shorter period of detention, a preference for the shorter path is gradually established. Hull (2) has sought to explain this phenomenon on the basis of the following hypotheses.

1. By the goal gradient hypothesis, the curve of sH_{R_1} rises more rapidly than that of sH_{R_2} , where R_1 and R_2 are the responses of taking the shorter and the longer paths respectively.

2. The fractional increment of sH_R at each reinforcement is the same in both curves, but the asymptotes are different.

3. Behavioral oscillation produces uncorrelated variations in sH_{R_1} and sH_{R_2} . The distribution of oscillation is assumed to be Gaussian, with the same standard deviation, σ_0 , for each habit. It is further assumed that σ_0 is unaffected by the magnitude of sH_R .

From these assumptions it follows that the distance between the means of the two oscillation distribution curves will increase with the number of reinforcements, and the amount of overlap of the two curves will diminish. If we know σ_0 , and the difference between sH_{R_1} and sH_{R_2} after any given number of reinforcements, the probability that the shorter path will be chosen at the next trial may be deduced from the normal probability integral. If

$$sH_{R_1} - sH_{R_2} = 2x,$$

then the required probability is given by

$$p = \int_x^{\infty} \frac{1}{\sigma\sqrt{2\pi}} e^{-x^2/2\sigma^2} dx.$$

This is the $\frac{1}{2}(1 + \alpha)$ of Sheppard's tables.

Conversely, if we know σ_0 and the proportions of choices of the shorter path at different numbers of reinforcements, the differences between the two habit strengths, measured in terms of σ_0 , may be read from the table of the probability integral. Even if σ_0 be unknown, we can obtain a series of values of x which are proportional to $sH_{R_1} - sH_{R_2}$.

Now, if all the above assumptions are justified, it follows that the deduced values of x will be the terms of a simple growth function with the same fractional increment as that of the two sH_R curves. For if

$$sH_{R_1} = M_1(1 - F^n),$$

and

$$sH_{R_2} = M_2(1 - F^n)^1,$$

then

$$\begin{aligned} x &= \frac{1}{2\sigma_0} (sH_{R_1} - sH_{R_2}) \\ &= \frac{1}{2\sigma_0} (M_1 - M_2)(1 - F^n). \end{aligned}$$

That is to say, x is a simple growth function of n , the number of re-

¹ In this equation F represents the complement of the fractional increment. That is, it is equivalent to $1 - F$ in Hull's equation for habit strength. The equation is identical with Hull's. (See 5.)

inforcements, its asymptote being $\frac{M_1 - M_2}{2\sigma_0}$, with the same F as the two habit curves.

It follows that if we can fit a simple growth function to the estimated values of x , some support will be afforded to the hypotheses. On the other hand, if no simple growth function can be fitted, one or other of the hypotheses must be abandoned or revised.

Hull's deductions from the hypotheses are less rigorous. They are contained in his corollaries III and IV, the latter of which states that "The preference for that one of a pair of acts involving the lesser delay in reinforcement is attained gradually as training increases." He finds support for this deduction in data published by Anderson and others, and prints a graph plotted from Anderson's data (2, p. 151). The number of experimental animals used by Anderson was rather small, and the data are consequently somewhat irregular. While his experiment is therefore sufficient to confirm Hull's rather cautious deduction, it is inadequate as a test for the more precise deduction.

Seward (4), using data published by Blodgett (1), has derived a theoretical curve to fit the percentage choices of the longer path in a simple rectangular maze, in which there were two paths

to the goal, one about three times as long as the other. He does not publish his equations, but he reports that the simple growth function which he at first tried for the curve of sH_R had to be replaced by another equation. This in itself suggests that some modification of Hull's hypotheses is required, but it seems unwise to introduce such a fundamental change as the abandonment of the hypothesis that habit strength is an increasing exponential function of the number of reinforcements. It may be useful to re-examine the data, and try whether some less drastic change can be made.

As a first step we may deduce the values of x from the observed percentages, or preferably from the values of Seward's theoretical curve, which seems to fit the data reasonably well. Table 1 gives the proportions of choices of the shorter path, as derived by measurement from Seward's curve, and the corresponding values of x , taken from the table of the normal probability integral.

It is at once obvious that x is not a simple growth function of n , for it shows, if anything, a tendency to positive acceleration instead of the negative acceleration that is characteristic of a simple growth function. And it follows therefore that we must abandon either the hypothesis that σ_0 is constant, or the hypothesis that the

TABLE 1

n	1	2	3	4	5	6	7	8
p	.5000	.5290	.5545	.5930	.6375	.6820	.7225	.7607
x	0	.073	.137	.235	.352	.473	.590	.709
n	9	10	11	12	13	14	15	16
p	.7990	.8347	.8630	.8868	.9120	.9287	.9453	.9554
x	.838	.973	1.094	1.210	1.353	1.466	1.600	1.700

two sH_R curves have the same value of F but different asymptotes, or both.

If we retain the hypothesis that σ_0 is constant, we have to find values of F_1 , F_2 , M_1 and M_2 such that $sH_{R_1} - sH_{R_2}$ is very close to a linear function of n , at least up to the first 16 terms, or, alternatively, that it is a positively accelerated increasing function of n up to about the 10th term, and thereafter shows slight negative acceleration. There are no values of these parameters which will yield a linear function, and the nearest approach to the alternative function is got if F_1 is very close to unity and F_2 much lower, say near .5, with $M_1 > M_2$. But this implies that the fractional increment at each reinforcement is very much greater in the case of the habit involving the longer delay, which is highly improbable.

We are therefore forced to conclude that, whether the two values of F are identical or not, the standard deviation of the oscillation function is not constant. The most plausible theory is that σ_0 is a diminishing function of sH_R , and as a first approximation we may assume it to be proportional to $100 - sH_R$ habits.

This of course implies some modification of the equation which expresses the proportion of choices of one of two available acts as a function of the difference between the two habit strengths. Let y and z be the two habit strengths at any stage in the training, and also the means of the two oscillation curves. Then, if the standard deviations are equal,

$$y - z = 2x,$$

where x is the distance, in standard measure, from either mean, of an ordinate which cuts off a proportion of the area of each curve equal to the proportion of choices of the shorter path. But if the standard deviations are un-

equal, then an ordinate that cuts off from each curve an area equal to the proportion of choices of the shorter path will divide the distance between the two means in proportion to the standard deviations. That is to say, the distance between the two means is given by

$$y - z = x(\sigma_{0y} + \sigma_{0z}),$$

where σ_{0y} and σ_{0z} are the standard deviations of oscillation of the two habits.

In the type of problem we are here considering, the only information we have is the values of p , the proportion of choices of the shorter path, at successive trials; and from these, with the aid of the table of the normal probability integral, we get the values of x . But we do not know the strengths of the two habits at successive trials, except at the beginning of the first trial, when both may be presumed to be zero; and we do not know the values of σ_0 , the standard deviation of oscillation, because these are assumed to be dependent on the values taken by y and z , the two habit strengths.

Our problem then is to find, for each trial, values of y , z , σ_{0y} , and σ_{0z} , which will yield the observed values of p , and which at the same time will satisfy the condition that

$$y = M_1(1 - F_1^{n-1}),$$

and

$$z = M_2(1 - F_2^{n-1}).$$

It should be noted that the values of y and z which determine the choice between the longer and shorter paths are the habit strengths as they exist at the *beginning* of a trial, and not after the reinforcement that comes at the end of a trial. Hence, in the equations for y and z above, the exponent of F is $n - 1$ and not n .

The only way to solve this problem is by successive approximation. The method actually adopted was as follows.

1. Trial values were assigned to M_1 , F_1 and σ_0 , the standard deviation of oscillation when habit strength is zero.

2. $y = M_1(1 - F_1^{n-1})$ was computed for four values of n , viz., 7, 10, 13 and 16.

3. For each of these values of n the observed proportion of choices of the shorter path was used to find x from the table of the normal probability integral.

4. For each of the four values of y , σ_0 was computed by

$$\sigma_0 = .01\sigma_0(100 - y).$$

5. The products $x\sigma_0$ were recorded.

6. The values of σ_0 were computed by

$$\sigma_0 = \frac{100 - (y - x\sigma_0)}{100/\sigma_0 - x}.$$

7. The products $x\sigma_0$ were recorded.

8. Test values of z were obtained by

$$z = y - (x\sigma_0 + x\sigma_0).$$

9. The differences between successive values of z' were recorded.

10. The ratio of the second of these differences to the first, and that of the third to the second, were recorded.

11(a). If these two ratios were unequal, it was clear that no simple

growth function could be found which would fit the whole series of z' values derived by the above method, and the process was begun again with a new set of trial values.

11(b). If the ratios were equal, or nearly equal, their cube roots were recorded. These indicate the value of F of a simple growth function that will fit the z' series.

12. The simple growth function that fits the z' series must be equal to zero when $n - 1 = 0$. This condition is satisfied if the four values of z' obtained by step 8 are proportional to the sums of the powers of F from 0 to 5, from 0 to 8, from 0 to 11, and from 0 to 14 respectively. These sums may be computed by

$$\sum_{s=0}^n F^s = \frac{1 - F^{n+1}}{1 - F},$$

or they may be obtained from the author's 'Tables to Facilitate the Fitting of Learning Curves' (5).

The numerical results of the operations described above, with the exception of step 12, applied to the set of trial values finally adopted, are set out in the successive columns of Table 2.

The sums of the powers of .84 from 0 to 5, 8, 11 and 14 respectively are 4.054, 4.949, 5.479 and 5.793; and the ratios of the values of z' to these sums are 11.25, 11.30, 11.33 and 11.34. The test referred to in step 12 above

TABLE 2

$$y = 82(1 - .85^{n-1})$$

$$\sigma_0 = 9$$

n	y	x	σ_0	$x\sigma_0$	σ_0	$x\sigma_0$	z'	d	Ratio	F
7	51.1	.590	4.40	2.60	4.39	2.89	45.61	10.29 6.16 3.64	.599 .591	.843 .839
10	63.0	.973	3.33	3.24	3.97	3.86	55.90			
13	70.3	1.353	2.67	3.62	3.42	4.62	62.06			
16	74.8	1.700	2.27	3.85	3.09	5.25	65.70			

TABLE 3

<i>n</i>	<i>y</i>	<i>z</i>	σ_{0y}	σ_{0z}	<i>x</i>	%	% (Seward)
1	0	0	9.00	9.00	0.000	50.0	50.0
2	12.30	11.30	7.89	7.98	0.063	52.5	52.9
3	22.76	20.79	6.95	7.13	0.140	55.6	55.5
4	31.64	28.77	6.15	6.41	0.229	59.1	59.3
5	39.20	35.46	5.47	5.81	0.332	63.0	63.8
6	45.62	41.09	4.89	5.30	0.445	67.2	68.2
7	51.07	45.81	4.40	4.88	0.567	71.5	72.3
8	55.71	49.78	3.99	4.52	0.697	75.7	76.1
9	59.66	53.12	3.63	4.22	0.833	79.8	79.9
10	63.01	55.92	3.33	3.97	0.971	83.4	83.5
11	65.86	58.27	3.07	3.76	1.111	86.7	86.3
12	68.28	60.25	2.85	3.58	1.249	89.4	88.7
13	70.34	61.91	2.67	3.43	1.382	91.7	91.2
14	72.09	63.30	2.51	3.30	1.513	93.5	92.9
15	73.57	64.48	2.38	3.20	1.629	94.8	94.5
16	74.84	65.46	2.26	3.11	1.747	96.0	95.5

is therefore satisfied. That is, an equation of the type

$$z = M_2(1 - .84^{n-1}),$$

will give a good fit to the calculated values of z . It only remains to determine the value of M_2 . Since M_2 is the limiting value of z as n tends to infinity, it can be got by multiplying the sum of the powers of .84 from 0 to infinity by the ratio 11.30. This gives

$$M_2 = 6.25 \times 11.30 = 70.6.$$

Table 3 presents the values of $y = 82(1 - .85^{n-1})$, $z = 70.6(1 - .84^{n-1})$, $\sigma_{0y} = .09(100 - y)$, $\sigma_{0z} = .09(100 - z)$, $x = \frac{y-z}{\sigma_{0y} + \sigma_{0z}}$, and the theoretical percentage of choices of the shorter path, that is, one hundred times $\frac{1}{2}(1 + \alpha_z)$. To these are added, for comparison, the percentages as measured from Seward's graph. There is a reasonably close agreement between the two series.

DISCUSSION

The first point to be noted is that the present reconstruction employs

nothing but rational equations, and does not involve any radical departure from the theoretical structure built up by Hull. In this respect it is therefore to be preferred to the reconstruction offered by Seward, who at one point substituted an *ad hoc* equation in place of a simple growth function. The only change that has been introduced is a refinement of Hull's conception of behavioral oscillation, but it is a refinement that has a rational basis. Now if a complex theoretical structure contains even one irrational element, agreement with empirical data cannot be regarded as affording support even to the rational elements. On the contrary, such agreement only throws doubt on the latter, at least until a rational basis can be found for the *ad hoc* principle that has been introduced. For if the rational principles cannot be made to generate results in conformity with observation unless they are distorted by the introduction of an irrational principle, their validity is called in question. If, on the other hand, the introduction of a fresh rational principle causes the whole theoretical structure to generate

results in conformity with observation, then there is confirmation not only of the new principle thus introduced, but of all the elements that co-operate to produce such results.

The results reported here therefore not only confirm the guess that behavior oscillation varies with habit strength, but add weight to the hypothesis that behavior is mediated by an intervening variable, sH_R , which increases exponentially with the number of reinforcements. And they also support Hull's guess that in conditions such as those of Blodgett's experiment the two habit strength curves have the same fractional increment.

This last hypothesis is explicitly stated by Hull in relation to another problem, the dependence of sH_R on the amount of the reinforcing agent. In that connection he writes: "Pending the appearance of more complete evidence, the working hypothesis is adopted that an increase in either the quality or the quantity of a reinforcing agent increases the rate of learning by raising the limit (m) to which the curve of habit strength approaches as an asymptote, the rate of approach (F) to this limit possibly remaining constant for all qualities and amounts of the reinforcing agent employed" (2, p. 133). A similar assumption is made, without further argument, in connection with the relation between habit strength and delay in reinforcement.

In the present investigation no initial assumption was made about the F values of the two habit strength curves, as that would have imposed undue restrictions on the search for a set of equations that would generate results closely approximating the empirical data. It is therefore of no small theoretical interest that the two values of F should turn out to be so near each other. The fact that the

fractional increment of the weaker habit is slightly greater than that of the stronger one may be regarded as objectionable, but it would not be difficult to write a set of equations with the same value of F , which would give a good fit to Blodgett's empirical data, even if the agreement with Seward's theoretical reconstruction were less striking.

Turning to the question of oscillation, we are at once struck by the apparent discrepancy between the results of the present study and the findings of Hull and his associates. The evidence he presents suggests that the range of behavioral oscillation is unaffected by the level of habit strength, whereas Blodgett's data can be accurately fitted only on the assumption that the range of oscillation is a diminishing function of habit strength. But before considering the significance of this discrepancy, we may note that everyday experience suggests that oscillation diminishes as habit strength increases. Hull mentions the striking of a wrong key on the typewriter as an example of oscillation, but a skilled operator will type dozens of pages without striking a wrong key. A virtuoso pianist will repeat the exposition of a sonata in such a way that the most careful listening reveals no deviation from the first performance. On the other hand, a less practiced performer is liable to vary in an unpredictable manner from one performance to another. No doubt accurate measurement would reveal variations in successive performances of the same work by a virtuoso, but the range of such variation is unquestionably very narrow.

Our problem, then, is to find an explanation of the fact that one group of experimental results demands the assumption that the range of oscillation is constant, and another that it is

variable. Hull's conclusion that the range is constant is based mainly on two pieces of evidence. The first is an experiment by Hays and Woodbury (2, p. 305) in which the pressure exerted by a hungry rat on the pressure bar of a Skinner apparatus was measured by a recording dynamometer. Two frequency distributions of those measurements are presented, one representing the performance of a rat after 400 reinforcements, the other after 800 reinforcements. The standard deviation of the second distribution is slightly greater than that of the first (6.9 as against 6.3), but this fact, taken alone, does not justify the view that the range of oscillation is unaffected by habit strength. For when the first set of measurements was taken, the minimum pressure that would suffice to deliver a food pellet was 21 grams, while in the second case it was 38 grams. The additional reactive inhibition thereby generated might be expected to add to the variability of the performance; and this expectation is confirmed by inspection of the distributions. The second one has a long tail on the negative side, stretching well below the threshold of release, whereas the first one is symmetrical. Apart from this long tail, the second distribution is more closely bunched around the mean than the first one is. An analogous result is produced when a moderately good pianist who has been accustomed to performing on an instrument with a light touch tries to play on a piano with a heavier touch. The additional effort required results in an increased variability of performance; but this variability diminishes with further practice on the heavier keyboard.

The second piece of evidence, quoted from *Mathematico-Deductive Theory of Role Learning* (3), is more convincing. The data are presented

in the form of the percentage of correct reproductions of nonsense syllables at 20 successive presentations of the cue syllables. If we assume that habit strength increases exponentially with the number of reinforcements, and that the percentage of successful reactions is determined by the distance between the reaction threshold and the mean reaction potential, in conjunction with the range of oscillation, it is impossible to explain the observed percentages unless we also assume that the standard deviation of oscillation is constant. The present author tried without success to reproduce the empirical data on the basis of the alternative hypothesis that oscillation diminishes as habit strength increases. And incidentally he found that a much higher standard deviation of oscillation had to be postulated than in the case of Blodgett's data.

A possible source of the discrepancy is suggested by one of the findings in a series of experiments on autokinetic movement conducted by K. Danziger in the psychological laboratory of the University of Cape Town. In the case of the majority of the subjects, who were volunteers, the variability of responses showed no tendency to diminish over a long series of observations; but in the case of a small number of subjects who were paid there was a striking decrease in variability. In itself this result is not conclusive, but at least it points to the necessity for taking careful consideration of the conditions of drive and incentive.

In this respect there is an important difference between rote memory experiments on human subjects and maze experiments on rats, in that reinforcement is secondary in the one case and primary in the other. In the former, the physiological drive that is active at the moment is unaffected by a successful response, but in the latter

it is substantially diminished. Now it is a well known fact that spontaneous activity, which is, essentially, variable behavior, is an increasing function of drive; and this suggests a refinement of our hypothesis regarding the relation between habit strength and the range of oscillation.

If behavior oscillation may be regarded as one aspect of the spontaneous activity initiated by a drive, then it follows that the range of oscillation will be contracted as a secondary consequence of any act that leads to a diminution of drive. But this suggests that the act of contracting the range of oscillation may itself be conditioned to a situation that leads directly to diminution of need. Thus when a hungry animal finds itself in a situation where it has previously discovered food, there is likely to be an immediate reduction in general irritability. It must be emphasized that what is postulated here is a generalized reduction of oscillation, applying equally to the whole range of behavior that may be elicited by a situation that is relevant to the satisfaction of some need. The contraction of oscillation of a single habit, which we have postulated as being a function of habit strength, may be regarded as a natural extension of the process to the particular acts which mediate the satisfaction of the need.

From this it would follow that in any learning process that proceeds entirely by secondary reinforcement, the range of oscillation will in general be greater than when reinforcement is primary, and there will be no contraction of oscillation as habit strength increases. In such a case general irritability is unaffected by the successful response to a stimulus, and this response therefore remains subject to all the variable influences which constitute the source of oscillation.

In the light of these considerations our hypothesis may be amended as follows: In any learning situation involving primary reinforcement there is an initial diminution of oscillation in consequence of the first reinforcement, and thereafter the standard deviation of oscillation is proportional to $100 - sH_R$ habs; when reinforcement is secondary, the standard deviation of oscillation remains constant at a level determined by the strength of the drive.

As a test of this hypothesis we may apply it to the same data used by Seward (4) to illustrate his theory of latent learning. Blodgett ran a group of hungry rats through the same rectangular maze to which reference is made above, but for the first 15 trials they received no food in the goal box. On the 16th and subsequent trials they found food. In the first 16 trials the proportion of choices of the shorter path increased slightly, though not nearly so rapidly as in the case of the control animals which were rewarded at each trial.

From the 16th to the 17th trials there was a sudden and very substantial increase in the proportion of choices of the shorter path, much larger than any single increase in the control group, and from then to the end of the experiment the increase was gradual. These results were interpreted by Blodgett as meaning that the animals had been learning throughout the unrewarded trials, but in such a manner that the learning had no objectively measurable consequences. He called it latent learning.

In point of fact the learning is not altogether latent, in that there is a very slow increase in the proportion of choices of the shorter path. This slow increase is typical of such experiments, and it is crucial for the understanding of the sudden leap following

the first rewarded trial. Obviously something is learned in the course of the unrewarded trials; and since there is no obvious source of secondary reinforcement, there must apparently be some diminution of a need, though clearly it is not the need for food.

A plausible answer to this puzzle is that the organism in the course of acquiring adaptive reactions to the objects through which its organic needs are satisfied must simultaneously learn to adapt itself to the physical and spatial features of the environment in which those objects are found. If, for example, it is being pursued by an enemy, its chances of survival are greater if it has already learned just where it can leap over an obstacle, where it can round a corner to put a barrier between itself and its pursuer, where it can creep into a hole. A full equipment of slick responses to every aspect of the environment is essential for success in such activities as the pursuit of prey and escape from danger.

Now such responses are relevant to any and every serious activity of the organism, and it is therefore to be expected that it will acquire a generalized habit of at once learning to move about easily and quickly in any new environment in which it finds itself. But a generalized habit must operate in much the same way as a drive, and we may describe it as a secondary drive. This drive is increased in strength by unfamiliar surroundings, and it is diminished as facility in moving about the new environment grows.

This means that the need associated with this drive is equally diminished by all the actions which adapt the organism to the spatial layout of the environment. Theoretically therefore there should be no differential strengthening of one response at the expense of the others, since all contribute

equally to need reduction. Nevertheless certain features of the new environment are more likely than others to have behavior directed towards them. For example, a hole that may serve as a refuge from a pursuing enemy, or a hiding place for food that is not immediately wanted, or a suitable place for a nest, is likely to become a focal point towards which exploratory behavior is chiefly directed. Or, in the case of a sophisticated laboratory rat engaged in an experiment on latent learning, the goal box may become the focal point of behavior simply because it is the place from which the animal is picked up by the experimenter and returned to the more familiar environment of the living cage, preparatory to feeding. The act which leads the animal by the shortest route to this point will be differentially strengthened.

In view of the secondary character of the drive that we assume to be operating here, it is to be expected (a) that the asymptotes of the learning curves will be substantially lower than in the case where there is primary reinforcement, and (b) that behavior oscillation will remain constant in range throughout the unrewarded trials. Further, since a strong hunger drive is in operation, it is likely that the range of oscillation will be relatively high. And finally, since the alternative reactions directed towards the goal box are assumed to be but a part of the secondarily motivated exploratory behavior evoked by the situation as a whole, and as such are therefore equally reinforced, it is to be expected that despite the differential reinforcement referred to in the last paragraph, the asymptotes of the two habit strength curves will be relatively closer to each other than in the case where both acts are primarily reinforced.

Starting with these premises, it is easy to write a set of equations which will generate results similar to those obtained by Blodgett in his latent learning experiments. The following is an example of such a set of equations.

A. Equations for the first 16 trials.

$$(1) \quad y = 70(1 - .84^{n-1})$$

$$(2) \quad z = 64.2(1 - .84^{n-1})$$

$$(3) \quad \sigma_0 = 13$$

$$(4) \quad x = \frac{y - z}{2\sigma_0}$$

$$(5) \quad P = \text{per cent choices of shorter path} \\ = 100 \cdot \frac{1}{2}(1 + \alpha_x).$$

B. Equations for trials 17 to 20

$$(6) \quad y = 64.88 + 17.12(1 - .84^{n-16})$$

$$(7) \quad z = 59.50 + 11.10(1 - .84^{n-16})$$

$$(8) \quad \sigma_0 = 9$$

$$(9) \quad \sigma_{0y} = .09(100 - y)$$

$$(10) \quad \sigma_{0z} = .09(100 - z)$$

$$(11) \quad x = \frac{y - z}{\sigma_{0y} + \sigma_{0z}}$$

$$(12) \quad P = 100 \cdot \frac{1}{2}(1 + \alpha_x).$$

The initial constants in equations (6) and (7) are the values of y and z in equations (1) and (2) when $n = 16$. The constants 17.12 and 11.10 were chosen on the basis of the assumption that after the introduction of reward, the asymptotes of the two learning curves would be the same as for the control group. Following Hull, and the evidence of the rote memory data referred to above, the standard deviation of oscillation at zero habit strength under conditions of secondary reinforcement is assumed to be 13; under conditions of primary reinforcement it is assumed to have the same value as in Blodgett's control experiment.

In the above set of equations the only parameters that were determined

in any way by reference to the empirical data were the upper limiting values of y and z in equations (1) and (2), and even these were determined to a certain extent by theoretical considerations. It is therefore significant that the values of P given by (5) and (12) correspond very closely to the empirical percentages. In the first 16 trials the calculated percentage rises slowly from 50 to 58.2. It jumps at the 17th trial to 83.9, and then rises more slowly to 93.6 at the 20th trial. Thus all the important features of the empirical data are reproduced. In addition to the slow initial rise and the sudden leap after the introduction of reward, we may note that (a) this leap carries the performance of the experimental group to a level which is significantly lower than that reached by the control group at the 16th trial; (b) the rate of increase from the 17th to the 20th trials is slightly greater than that of the control group at the same level of performance (*cf.* Table 3, trials 10 to 14); and (c) at the 20th trial the experimental animals have still not reached the level of the control group at their 16th trial. In all these respects theory and observation agree.

SUMMARY

In experimental situations presenting alternative paths to the goal, unequal in length, preference for the shorter path is gradually established. One of the theoretical constructs by which Hull explains this phenomenon is behavioral oscillation, which he assumes to have a constant standard deviation. It is shown, by the analysis of data published by Blodgett, that this constancy hypothesis is untenable. The empirical measurements can be reproduced only by (a) abandoning the hypothesis that habit strength is an increasing exponential

function of the number of reinforcements, as Seward does, or by (b) assuming that the standard deviation of oscillation is a diminishing function of habit strength, while leaving the other basic concepts of Hull's theoretical system intact. A method of deriving equations giving expression to the latter assumption is presented, and it is shown that such equations fit the experimental data adequately.

It is further argued that if oscillation be regarded as an aspect of the spontaneous activity initiated by a drive, its standard deviation is likely to remain constant in situations where learning proceeds by secondary reinforcement, and to be a diminishing function of habit strength where reinforcement is primary. This hypothesis is supported by comparison of rote memory, involving secondary reinforcement, with the Blodgett data referred to above. A more critical test is afforded by an analysis of a latent learning experiment reported

by Blodgett, in which the reinforcement was secondary during the first 15 trials and thereafter primary. It is shown that a set of equations giving expression to the hypothesis yields results that in every respect agree with the empirical data.

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MOTIVATION, PERCEPTION AND ATTITUDE CHANGE

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Perception is a central problem in social psychology. In this paper attention is drawn to the manner in which past experience and motivation influence perception in social situations, and, in this way, influence our social attitudes. Any study of attitudes must take into account the fact that attitudes have a functional value, and that a person perceives social situations in terms of his own past experiences, values and purposes.

PERCEPTION INFLUENCED BY PAST EXPERIENCE AND MOTIVATION

We know that an automobile is perceived as an automobile rather than as the unclassified series of light waves which produces the retinal stimulus. Moreover, different people perceive the same automobile in different ways: the dealer perceives it in terms of its age, make, and sales price; the purchaser sees it in terms of its cost and future use. Purposes and past experience are both known to play a part in this perceptual process.

The view held here is that perception is an active purposive process developed through past experience. Although the role of conscious processes usually referred to as interpretation and judgment is clearly recognized as a factor in some perceptions, it is felt that an even greater emphasis should be given to the essentially unconscious selective aspect of all perception.

It is commonly recognized that in the adaptation of the organism to its environment, perception plays an important role. Those cues which aid the individual in adjusting to his environment are likely to be the ones most

quickly learned and longest retained, even though they may not be consciously recognized. In this sense, perception may well be governed by the same general laws applicable to any learning process. The greater pragmatic value for most people of such a distance cue as size as compared with brightness seems to be the most reasonable explanation for the dominance of size over brightness as a distance cue when these two are placed in conflict (3).

Helmholtz called attention to the experiential and purposive nature of perception in observing that we are not aware of such sensory phenomena as the blind spot, the Purkinje phenomenon and combination tones. His conclusions could well have appeared in a present-day psychological journal:

"We are not in the habit of observing our sensations accurately, except as they are useful in enabling us to recognize external objects. On the contrary, we are wont to disregard all those parts of the sensations that are of no importance so far as external objects are concerned" (7, p. 6).

There is growing evidence that we learn to perceive, and that perception, like learning, is purposive. Anthropologists have reported many instances which suggest that perceptions serve the values and purposes acquired in a cultural group. Experimental studies by psychologists in recent years give strong support to these observations¹

¹ A very recent study by Riesen (10) provides evidence for the thesis that perception is a learned response. Riesen raised two chimpanzees in complete darkness up to the age of sixteen months. A summary of his ob-

(8, Chap. 3; 9, Chap. 15). Our unique and common needs, values and purposes have been shown to exercise an active functional influence over both what is perceived and how it is perceived.

In the past there has been a tendency to explain these differences in observation only in terms of conscious judgments and interpretations. The word *see* has often appeared in quotes, implying that the stimulus is seen in one way and consciously interpreted in another. The work of the Hanover Institute suggests strongly that perception itself is an unconscious judgmental process—that interpretation occurs at an unconscious, as well as at the conscious, level—and that the *see* may be taken out of quotes.

Past experience and purpose cannot be clearly delineated as controlling factors in the perceptual process. A situation can have meaning to the individual only if he can relate it to his purposes *through* past experience. Purposes are acquired, and in that way represent the conclusions of past experience. The purposive actions of today are evolved from the reward-punishment experiences of yesterday, and to speak of the results of experience in this sense is to speak of acquired purposes. As we learn through experience to perceive, so we learn through experience to evaluate. Thus our present purposes become both functions and representations of our previous successful and unsuccessful activities.

Ames, of the Hanover Institute, has stated his position with respect to this issue as follows:

"It is apparent that there is no 'meaning,' for instance, in the undifferentiated observations suggests that these primates appeared to be blind for most practical purposes when first brought into the light.

light rays themselves which impinge on the cornea. Nor is there any 'meaning' in the light rays as differentiated by the lens system of our eyes that impinge on the receptors in our retinas; or in the electrical-chemical disturbances that take place between the retinal receptors and the visual center of the brain. 'Meaning' is significance which has been disclosed through prior purposeful action. The significance is related to stimulus patterns existing at the time of the original experience and is reactivated when the organism is later subjected to similar stimulus patterns.

"The function of sensations is not to disclose the innate character of a thing as such or its spatial position as such. It is to establish between the evolving organism and the ever-changing environment a relationship on the basis of which the organism may effectively carry out its purpose. This means that sensations are prognostic directives for purposeful action" (2, p. 3 f.).

Ames has developed over sixty demonstrations concerned with the nature of visual perception. Since his conclusions are derived mainly from these demonstrations, it seems pertinent to discuss briefly two of them.

One of the demonstrations is a distorted room so designed that when you look at it from a particular point of view, it gives rise to the same unicocular indications that you would get from a rectilinear room. This room is placed behind a screen so that you can see it only with your head in the position from which it appears to you as a rectilinear room.

"If you look at the room with one eye from that point of view, your sensations will be of a rectilinear room. You could look at the room forever and, for all that your sensations give you, you would still believe it was a rectilinear room" (3, p. 57 f.).

Preliminary studies at Princeton² of subjects viewing the distorted room reveal considerable individual variation in perception. The distorted room used was not perfect; minor monocular cues still existed and, if perceived, could "tip off" the observer that the room was distorted. These cues included differences in lighting, detail, and minor imperfections in construction. If a subject, viewing the room monocularly, perceived one of these minor cues, it seemed to lead him to discover other evidences of distortion. Some subjects remained unaware of any distortion for relatively long periods of time. Observation of a few subjects, blind in one eye for five years or more, suggests the possibility that they are especially sensitive to monocular cues. This greater discrimination and selectivity on the part of persons with vision in only one eye can be explained in terms of the necessity for the learning and using of these cues for the purpose of adapting to an environment in which accurate judgment of size and distance is so important.

Another demonstration consists of a rectilinear room with one open wall. The inner walls of this room (including the floor and ceiling) are covered with oak leaves. The subject views the room from the open side. With normal vision the room looks perfectly rectilinear, but when the subject views the room through aniseikonic lenses, the room takes on a distorted shape. These lenses alter in specific ways the geometric character of the room, depending on the locus of fixation (*e.g.*, the walls, the floors, etc.). This phenomenon cannot be explained entirely on the basis of optics, because when the same lenses are worn while looking at a room

containing such familiar objects as desks, chairs, people, etc., almost no distortions are seen at first unless specifically pointed out by the demonstrator.

In this demonstration, which is somewhat analogous to an early study by Adams (1), an individual views two situations differing primarily in that one is much more familiar to him than the other. In both instances the visual stimulus patterns are distorted, but in the less familiar situation this distortion is observed more quickly and more severely. Thus past experience has an immediate influence upon the perceptual response.

In this section we have attempted to point out briefly that perception is an active process determined by past experience and purpose. The physical stimulus and the physiological state of the organism are also obvious conditions to consider in accounting for individual differences. The evidence suggests that perception takes place in an acquired referential frame, and that situations can have meaning to the individual only in terms of his purposes within such a frame of reference. So viewed, purposes are largely functions of past experience which perform a selective action in the current perceptual situation.

IMPLICATIONS FOR ATTITUDE FORMATION AND CHANGE

If our individual and social purposes are important determinants of what is perceived, as has been suggested above, many of the basic problems of attitude formation, persistence, and change can be seen in a new perspective. It becomes clear that differences in the evaluation of a social situation are not differences in conscious judgment and interpretation alone. Prior to these conscious judgments and interpretations, there very well may be more ba-

²The Hanover Institute has provided a complete set of these demonstrations for the Office of Public Opinion Research, Department of Psychology, Princeton University.

sic differences in the manner in which situations are perceived, depending on differences in past experience and purpose. Fundamental attitude changes are not likely to occur unless there are changes in the individual and social purposes which often play an unconscious selective role in perception. It follows from this thesis that attitudes are functionally related to values and purposes, that a shake-up in the identifications and in the acquired status-strivings or purposes which accompany these identifications is necessary if these attitude changes are to occur (11).

The attorneys for the N. A. M. and the C. I. O. in debating about the provisions of the Taft-Hartley Bill and the probable effect of its passage on the public welfare really have no parallel topics for debate. They neither perceive the same Taft-Hartley Bill nor the same public welfare. Since the disagreements are more fundamental than differences in conscious judgments or interpretations, no amount of debate is likely to be effective. These judgments or interpretations may change, but they will still remain judgments or interpretations of differently perceived situations, unless one or the other of the attorneys first undergoes a complete breakdown and reorientation of his identifications and related strivings. In such a case he could no longer effectively represent his group.

If the N. A. M. and the C. I. O. attorneys should honestly attempt to compromise on their points of disagreement, they would not be likely to achieve full success. For the concessions of each would be limited by the framework of the social situation he perceives. Each would unwittingly weigh the other's objectives in terms of his own purposes. What appears to be a minor demand to one may strike at the core of the other's purposes, or may not even come within the framework of the other's perception

of the situation. No amount of good will could reconcile the differences in the way they would perceive the same social situation.³

The completely different economic and social circumstances attending the development of a middle class child give growth to ego-strivings which do not permit him to perceive a social situation in the same way as a working class child perceives it. The values and purposes acquired in childhood influence both his perception of social situations and his attitudes concerning them. This may in part account for the differences in attitude reported between people in America who identify themselves with the middle class and those who identify themselves with the working class (6).

In this sense class differences, whether consciously recognized or not, appear to be fundamental differences in acquired purposes, as well as in economics and attitudes. Persons who have acquired their status-strivings in the socio-economic atmosphere of one class are believed incapable by reason of past experience of perceiving the goals of another class. The difference is again thought to be one of perception as well as conscious interpretation and judgment.

³ President Roosevelt well appreciated this point when he said in his message to Congress (April, 1938): "Government can deal and should deal with blindly selfish men. But that is a comparatively small part—the easier part of our problem. The larger, more important and more difficult part of our problem is to deal with men who are not selfish and who are good citizens, but who cannot see the social and economic consequences of their actions in a modern, economically interdependent community. They fail to grasp the significance of some of our most vital social and economic problems because they see them only in the light of their own personal experience and not in perspective with the experience of other men and other industries. They, therefore, fail to see these problems for the nation as a whole."

This difference makes the crossing of the class lines during a single generation an extremely difficult psychological as well as economic and social problem. At times, however, the attendant uncertainty and insecurity may result in the complete breakdown of the ego and a satisfactory reorientation of group identifications may occur.

It has been suggested that:

"In periods of relative instability and rapid transition brought about by technological developments, crises in social organization (such as depressions, etc.), personal conflicts are likely to become both more widespread and more intense. An individual's loyalties and identifications are, in such a period, torn farther apart. Old loyalties may prove illusory, reference or membership groups that have provided status may split or disintegrate. In such times, the individual seeks new identifications and loyalties" (5, p. 52).

This thesis holds, then, that changes in group identifications and status-strivings (acquired purposes) are essential to any relatively lasting attitudinal change. If existing identifications are sufficiently constant and satisfying in terms of ego-strivings for status, lasting changes in attitudes seem improbable. If existing identifications are not satisfying in terms of ego-strivings, however, a person may discard his previous identifications and attendant attitudes, identify himself more or less permanently with some new group or groups, and become personally involved in the purposes of the new group.

In general such re-identifications appear to be facilitated by changing conditions in the individual or his environment which lead to conflicts of identifications. As Sherif and Cantril point out, re-formations of the ego may occur during such critical periods as adolescence, when the existing identifications are rendered particularly unstable.

"Changes brought about by sexual maturity, by significant bodily developments with their accompanying serious effects on attitudes of masculinity and femininity, shifts (or preparation for shifts) in economic roles, and actual or more seriously anticipated shifts in social status, all make the period of adolescence a crucial stage of psychological transition involving ego problems" (11, p. 201).

Breakdowns in the fully developed ego may also occur when some radical change occurs in an individual's environment, such as brought about by war, death, migration, or economic changes. The individual may be forcibly removed from the members and symbols of the group with which he identifies himself. The resulting uncertainties and insecurities may foster the acceptance of new standards, values, and identifications, and in that way, new and lasting attitudes.

At times people of widely different ideologies have been drawn together in support of common purposes. The America First Committee had extremists from both right and left among its membership prior to the German attack on the U. S. S. R. Each group was publicly embarrassed by the presence of the other, but, by reason of their temporary purposes, supported one another's arguments. The underlying purposes of the two groups were in conflict even though the situation caused both groups to engage in the same activity. Consequently when the situation changed, the basic conflict in underlying purposes again became apparent.

Such situations may also cause temporary changes in attitude when individuals with conflicting loyalties share purposes and are joined in action to further these temporary purposes. In some situations during the war, white and colored soldiers united for the common purpose of winning; strong

group identifications emerged. Racial differences seemed relatively unimportant. When the common purpose was achieved, however, prejudices quickly returned. In other words, the attitude changes observed seemed to be specific to the purposeful action situation, and did not endure beyond this situation.

"Outstanding illustrations of the power of working together on common problems have come from the fighting fronts. In some cases the segregation imposed by the Army and Navy broke down. White soldiers and Negroes, fighting side by side and owing their lives to the bravery of their companions, achieved splendid solidarity. Distinctions between Catholics and Protestants, between Jew and Gentile, mean little to a bomber crew, a submarine crew, or an advancing patrol. Each man rates on his merits and his service to the group. Common projects required interdependence and fostered appreciation" (12, p. 61).

In situations involving common purposes and action, such as the war situation, perceptions were changed. For example, the Negro was perceived more as a cooperating fighting soldier than as the member of an ethnic out-group.

Individual strivings for status still occurred in the war situation, but they were overshadowed in importance by major purposes of the identification group. The major purposes of the individual became as one with social purposes of the group, while still being retained as individual purposes by the members of the group (11). The change in group status was a change in status for every member of the group. Removal of the need for group status-strivings left the members of the group with their old status-strivings; individual conflicts and prejudices returned.

A similar situation may occur under normal peacetime conditions. A child may choose a colored playmate in a

particular situation because this selection will help him to achieve his purposes in terms of the immediate situation. When the colored playmate no longer helps him to satisfy his purposes after he returns to his old group, the child will no longer select the Negro as a companion. And if the prejudice is the norm of his group, he will not be inconsistent in adopting this norm.

It would seem, therefore, that a greater appreciation of the important relationship of motivation to both perception and attitudes may lead to more productive research on the problem of attitude change. Concentration on the problem of altering an individual's identifications, and thus many of his social purposes, would seem to be getting at both the manner in which a person perceives a situation and his attitude towards that situation.

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SOME PSYCHOLOGICAL IMPLICATIONS OF CORTICAL SUPPRESSOR AREAS

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The discovery of suppressor areas in the cerebral cortex, and the subsequent neurophysiological work done upon them have implications for the psychologist which it would seem well to make explicit. The specific implications to be mentioned here—implications concerning the effects of relaxation, of the use of pain in learning experiments, and the action of pain in intersensory induction—undoubtedly do not exhaust the subject. They should, nevertheless, serve to sharpen the awareness of the psychologist for one group of highly interesting and informative results being obtained by neurophysiologists.

The salient neurophysiological results will be outlined first. A region between the motor and premotor areas of the cortex which, when stimulated, brought about relaxation of the contralateral musculature was reported first by Hines (4). This work was confirmed and extended by Dusser de Barenne, Garol, and McCulloch (2) both by means of "physiological neuronography" (local application of strychnine to the cortex) and electrical stimulation. A total of four suppressor areas have been demonstrated in the cerebral cortex. These are known as 2s, 4s, 8s, and 19s. Stimulation of these areas results in reduced excitability of the motor cortex. The evidence of the suppressor action includes a reduced level of electrical activity in the area suppressed, and also a rise in the threshold for electrical stimulation. This suppressor action involves circuits through subcortical structures, the details of which are not of concern here.

Of considerable interest to psychologists are two recent papers, one by Gellhorn (3), the other by Barker and Gellhorn (1). In these it was demonstrated that nociceptive impulses resulted in the arousal of suppressor areas, and further that excitation of the suppressor areas either diminished or suppressed the cortical effects of afferent impulses. When stimulation of a nociceptive nature was sufficiently strong to arouse autonomic reflexes the suppressor areas were activated. Therefore, we may assume that nociceptive stimulation will lead to an arousal of the suppressor areas, and that this will in turn lead to suppression of the sensory cortex. These results are therefore supplementary to the earlier work on suppression of the motor cortex, and demonstrate a similar arrangement in the sensory system.

These, then, are the bare neurophysiological results. It would seem reasonable to relate them to the three psychological topics previously noted, since in each case implications may be found which cast new light on the processes involved, and in one case call into question a rather generally used technique in learning experiments.

One of the stronger arguments for a proprioceptive theory of vigilance has been the rise in sensory threshold when the subject is relaxed (7). Therefore, it is argued, proprioceptive discharge will increase the excitability of the sensory cortex, and "progressive relaxation" (5), by causing a diminution in the amount of this discharge, leads to a rise in threshold. But the work on suppressor areas permits another interpre-

tation. We may think of progressive relaxation, not as the absence of activity in the motor cortex, but rather as the development of cortical suppression, probably by domination of the cortical motor areas by the suppressor areas. Since it has been demonstrated that arousal of the cortical suppressor areas will reduce the activity of the cortical sensory areas, then thresholds must be raised. Furthermore, there is no reason to suppose that learning does not take place for suppressor functions as well as excitatory ones, and practice in relaxation may then be considered to result in learning to "suppress." The central point is that motor and sensory suppression can take place simultaneously, and hence the reduced muscular activity involved may be taken as evidence of the dominance of suppressor mechanisms, rather than being itself a link in the causal chain leading to higher thresholds.

We need not assume, of course, that proprioception plays *no* role. Probably the recircuited proprioception resulting from muscular contraction has an effect on the central nervous system, but there is no reason to assume that proprioception is an unique or super-sense. This point has been commented on elsewhere (6).

The use of pain as either punisher or "emphasizer" has been popular in learning experiments. But the demonstrated arousal of suppressor areas by "painful" stimulation casts a new light on this technique. It would now seem that pain has two aspects: a sensory one, in the same meaning as touch, warmth, and so on, and a suppressor-arousing one. Furthermore, it will be remembered that the suppressor-arousing function of pain arises only when there is sufficient strength of effect to arouse also visceral responses. Hence, we may hypothesize that weak pain and strong pain will have different effects (*cf.* Tol-

man, Hall, and Bretnall, 9). And it further follows that as an organism develops negative adaptation, or habituation, to repeated painful stimulation, the neurophysiological effect and thus the psychological effect of the stimulation is changing. The signal or emphasize quality may then become more important than the disruptive, suppressor quality.

We may also understand why intense pain should be so disruptive. As cortical suppression builds up, release phenomena appear, and with intense pain the organism may become functionally almost decorticate. Finally, fainting or coma may intervene. Giving a strong electric shock in a learning experiment, we may conclude, does a good deal more than produce an "unsatisfying" state of affairs. Arousing suppressor areas in this way introduces confounding factors into the situation which will interfere with the interpretation of the results.

Intersensory induction is another topic for which cortical suppression has importance. This topic has been reviewed by Ryan (8), and in general it is found that stimulation in one sense department will lower thresholds in another, although the evidence is not unequivocal. Thus it is usually true that a tone sounding simultaneously with the presentation of a gray will cause the gray to appear brighter than when presented in silence, and similar effects may be expected for other pairs of stimulus. But pain should behave somewhat differently.

When a mildly painful stimulus is used as the inducing stimulus, then positive induction should be found, that is, thresholds should be lowered in much the same way that they would be lowered by, say, a tone. When, however, a stronger painful stimulus is used, at some point the suppressor effect should take control, and induction become negative. This follows from

the previous discussion of the double aspect of pain.

The issue will be confounded, however, by the fact that pain is virtually never aroused alone, but is accompanied by activity in other sense departments. There will be pressure if arousal of pain is by a needle, warmth if heat is the stimulus, and so on. Careful experimental separation of these effects is therefore necessary.

No doubt there are other implications of the suppressor work which have not been considered here. Also the neurophysiologist has not yet said the last word on this topic, and some adjustment of detail, or extension of implication, may be possible when further results are in. Nevertheless the work on suppressor areas is sufficiently well established to force the psychologist to pause and check his own experiments before deciding that the implications may be ignored.

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FOOD-SEEKING DRIVE, AFFECTIVE PROCESS, AND LEARNING

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The purpose of the present paper is to consider some of the theoretical implications of the writer's experiments upon food acceptance (21, 22, 23, 24, 25, 26, 28, 29, 30, 31, 32, 33, 34, 35, 37, 38, 39) and the relation of these experiments to current views concerning drive, affective process, and learning. If the reader wishes to study the background literature, much of which is taken for granted here, he is referred to the bibliographies of published critical reviews (27, 36).

THE WRITER'S EXPERIMENTS UPON FOOD ACCEPTANCE

The writer's first observations upon food preference were made in Berlin in 1927 in an attempt to escape the limitations of a purely introspective study of the affective processes (21). At that time it was our hope to find a sound objective basis for the analysis of affectivity and eventually to discover in brain dynamics the physiological equivalent of pleasantness and unpleasantness. A sound objective basis for affective psychology has been revealed by the work of many investigators, but the brain dynamics of affective arousal remain as a puzzle.

Our second group of experiments upon food preference (22, 23, 24) confirmed the findings of the first work. In general, we found a remarkable uniformity and stability in the preferential selections of rats when the animals are maintained upon a constant diet. Test-foods arrange themselves into transitive series, or hierarchies, symbolized as follows: $A > B > C > D > E > F$. A given preference may not be apparent

at the start of a test but with continued testing a preferential trend appears. Rats *learn* to make preferential discriminations.

Hierarchies of preferences are remarkably stable. The percentage of choices which indicates a preference increases with practice in discrimination. This percentage decreases during sickness and under extreme deprivation (33). It varies somewhat with the form of apparatus employed in testing (31). But despite these variations in the percentage of choices showing a preference, the hierarchy of preferences itself remains surprisingly constant.

We first found evidence that the hierarchy of food preferences can be altered experimentally in work upon the reversal of preferences through controlled pre-feeding (26). This experiment showed clearly that a given preference, $A > B$, can be reversed by permitting rats to ingest A for a controlled period of time prior to tests of preference. The change from $A > B$ to $B > A$ did not come at once; it came gradually with practice. When pre-feeding was discontinued there was not an immediate return to the original preference but only a gradual preferential trend in that direction. If food preferences are determined solely by internal chemical conditions, we argued, these reversals of preference should occur immediately. The fact that several days of training were required to change a preference indicates that rats have to *learn* to make preferential discriminations. But the *kind* of preference which the animals consistently learn, whether $A > B$

or $B > A$, is dependent upon intra-organic conditions rather than practice.

A further investigation confirmed the fact that the percentage of preference depends upon practice as well as upon the intra-organic chemical state (28). All of the early work, in fact, emphasized two interdependent conditions of food preference—(a) practice and (b) the chemical state of the organism as regulated through the diet.

A major advance came with the introduction of self-selection maintenance and the exact chemical control of the diet (29). These changes in the conditions of maintenance were made in the hope of discovering the relation between our earlier findings and the results obtained by Richter (27).

In the first major experiment combining self-selection maintenance with preference testing, Young and Chaplin (37) found a surprising result. An attempt was made to reverse a preference $A > B$, by creating a need for B through dietary deprivation. It was found that despite prolonged deprivation of B, and with marked signs of bodily need for B, the rats continued to accept A in preference to B. A new technique of testing preference was then tried. With the new technique the reverse preference, $B > A$, at once developed! For a few days the rats exhibited simultaneously two opposed preferences! When placed in the apparatus with which they had first been trained (test-foods side by side and relative positions interchanged from trial to trial) the rats preferred A to B; but when placed in a new apparatus (test-foods widely separated as in a Y-maze and relative positions fixed throughout the test) they preferred B to A. Here was an obvious exception to the rule that test-foods arrange themselves into a transitive series! Here also was a problem for any one who maintains that rats select foods in accordance with bodily

needs, since one choice agreed with bodily needs and the other did not!

The interpretation of the Young-Chaplin result became a matter of critical importance for the theory of food acceptance. Our first explanation of the result was that there are two kinds of food preference: one based upon palatability and determined by the head receptors ($A > B$) and the other based upon organic need and dependent upon chemical conditions within the organism ($B > A$). We assumed that a choice necessarily made on the sole basis of the head receptors might differ from one made when the head receptors could not function as determiners of choice (by having the test-foods out of range of the head receptors at the point of choice). This explanation of the two simultaneous preferences, however, was necessarily abandoned after a tedious series of control experiments. The validity of a distinction between palatability and appetite has been demonstrated in more recent experiments, but the distinction was not validly demonstrated by the work of Young and Chaplin.

Control experiments proved conclusively that the Young-Chaplin result did not depend upon the method of testing (31). When the amount and distribution of practice was controlled and the diet held constant the two methods of testing (foods-together and foods-apart) yielded precisely the same kind of food preference, although the degree to which a preference was revealed (as shown by the percentage of preference) varied somewhat with the kind of apparatus employed. Despite several attempts, we were unable to demonstrate one preference with one kind of apparatus and the opposite preference with another *when the factor of practice was held constant*. Returning to the Young-Chaplin result, then, we were forced to the conclusion that one kind of prefer-

ence, $A > B$, revealed a persistent preferential habit based upon the *original* relationships of palatability and that the other kind of preference, $B > A$, rested upon some factor or factors other than habit. Inasmuch as the newly forming preferential habit agreed completely with known and manifest bodily need, in this and in a similar experiment upon hunger and thirst (30), we concluded that *preferential feeding habits tend to form in agreement with bodily needs, but established feeding habits may persist regardless of bodily needs*. A preferential feeding habit, in fact, may obscure some existing need.

The final interpretation of the Young-Chaplin result confirmed the view that feeding habits alone may determine the preferential selection of food. Importantly, however, the chemical condition of the organism as determined by the diet, regulates the initial choice.

Our research bifurcated at this point. One line of investigation was directed toward the analysis of habit as a determiner of choice (32, 33, 34). The other line of work was turned toward the chemical and intra-organic factors (other than habit) which regulate the selection of food (35, 38).

Experiments upon the habit factor have culminated in the finding that the rate of learning is not dependent upon the palatability of the food which is used as an incentive (33, 34). We have found no evidence for the view that rats *learn faster* when offered a highly palatable food than when offered a food of low palatability, *provided the frequency and distribution of nibbles of food are the same for the two incentives*. At all stages of practice, however, rats *run faster* to reach a highly palatable food than to reach one of low palatability. Further, there is less day-to-day variance with the more palatable food. Also rats delay less in the presence of the highly palatable food before ac-

cepting it. If one thinks in terms of the total performance, there is no doubt that the level of performance is higher with a highly palatable reward (sugar) than with one which is less palatable (casein) *at all levels of practice*.

The second line of investigation has been concerned with determinants of food acceptance other than practice, or training. This work has shown the necessity of distinguishing between intra-organic and environmental determinants of food acceptance (35, 38). The selection of food by the rat clearly depends upon the characteristics of the foodstuff. For example, if solutions of sodium chloride are presented under stable conditions of maintenance, the animals show a marked preference for solutions with a concentration of approximately 0.7 per cent. This optimal concentration is not significantly altered by the dietary deprivation of sodium chloride nor even by surgical removal of the adrenal glands.

The evidence which is now at hand leaves little doubt that the level of acceptability depends directly upon characteristics of the food object—its kind, the concentration of solution, its temperature, texture, etc. The term *palatability* is commonly used within the science of nutrition to refer to the fact that the relative acceptance of food is dependent upon the characteristics of the foodstuff. This concept is even more important in psychological analysis than in nutritional investigations.

When the organic state (and hence appetite) is held constant and when the factor of habit is controlled, the selection of food still varies markedly with the properties of the foodstuff (palatability). Hence instead of the two factors which our early work disclosed (habit and biochemical state of the organism) we must henceforth reckon with three: (a) habit and (b) the

chemical state of the organism and (c) the characteristics of the foodstuff.

The present analysis is based mainly upon our experimental findings. The aim of this study is to find how our results relate to fundamental concepts of motivation, affectivity, and learning. Although the laboratory findings can definitely be stated in terms which are free from hypothesis, the findings are here used as a springboard for interpretation and theory.

DO ANIMALS SELECT FOODS WHICH THEY NEED OR FOODS WHICH THEY LIKE?

Bodily need is an objective nutritional concept which can be defined without reference to behavior. Need is shown by certain symptoms of depletion such as retardation of growth, failure to reproduce, loss of hair, softening of the bones, failure to maintain a homeostasis, or by death itself. Operationally, *need* can be defined in terms of specific or general dietary deprivations and the associated symptoms or syndromes which appear after specific periods of partial or total deprivation. There are, of course, psychological manifestations of need as well as structural changes: increase or decrease in the average level of food intake, increase or decrease in the level of activity, defective vision, retardation in performance upon the maze, changes in food preferences, etc. These and other manifestations of need have been studied by psychologists. Further references to the behavioral manifestations of need are frequently met in the nutritional literature.

Some psychologists have used the term *need* as practically synonymous with *drive* or *motivation*. This usage is misleading since a *drive*, in the psychological sense, is something very different from the dietary requirements as specified by a nutritionist.

Richter (15, 16) has argued that the maintaining of homeostasis is a fundamental process and that self-regulation includes the mechanisms of behavior. When there is need for a specific substance in order to maintain homeostasis the rat becomes sensitized to the substance required. The animal is able to select and balance his diet according to bodily needs and his food selections are, to a considerable extent, a dependable index of those needs.

Our work supports Richter's contention that to a considerable extent food selection is an index of bodily need. We have found, for example, that a food preference can be reversed either by satiating a rat upon the preferred food or by depriving him of the non-preferred food. Satiation, presumably, removes a need by reducing it to zero, thus meeting it completely. Deprivation of some substance which is essential for growth or reproduction or health or for survival itself builds up a bodily need for that substance. Thus food preferences change in agreement with bodily needs.

There are, however, a good many impressive facts which indicate that food selection is *not* a dependable guide to the existence of bodily needs and that factors other than need are important determinants of food acceptance. Some of the facts which present difficulties to the theory that food selections reveal needs are summarized in the following generalizations:

1. Marked food preferences develop when there is no known metabolic need and when growth and health are normal. If rats are maintained upon a fully adequate diet, with an unlimited supply of solid food and water in their cages at all times, they still reveal marked food preferences among test-foods which are supplementary to the main diet (34). Where no known metabolic need exists one can hardly

appeal to some need in explanation of the facts.

2. Rats accept with avidity a substance which meets no metabolic need. Hausmann (10), Beebe-Center, *et al.* (3) have shown that rats prefer water sweetened with saccharin to plain water. Saccharin, as we know, has no value in nutrition; it passes through the body unaltered and it could not possibly meet a metabolic need. A direct explanation of the facts in terms of bodily need would be superfluous and misleading.

3. The quantity of a particular foodstuff ingested varies markedly with its characteristics such as temperature, concentration of solution, texture, and the like, as well as with the surroundings of the foodstuff which are not directly related to intrinsic properties. If bodily need alone determined food acceptance, one would expect to find fairly constant quantities of a substance ingested per day per animal. But when solutions of sucrose or of sodium chloride are presented in different concentrations, the daily intake of these substances and of the total solution varies markedly with the concentration (35). In fact, we can regulate the intake of certain substances, to some extent, by varying the concentration.

4. If foodstuffs are presented under optimal conditions, an animal may eat to excess. Nelson (13) has reported that when rats can obtain sodium chloride at a concentration of 0.8 per cent (which is very near our observed optimal concentration) they ingest great quantities of it. Under these conditions their growth is retarded, their kidneys and other organs become enlarged, and there are doubtless other symptoms of an excessive intake of sodium chloride. In other words, rats may take much more of a food than the quantity required to maintain homeostasis when the food is obtainable under optimal

conditions. It may prove possible, when palatability is adequately controlled, to study diseases of excess as we now study deficiency conditions.

5. Rats sometimes fail to select foods in agreement with known bodily needs. For example, when deprived of vitamin D, rachitic rats failed to show a preference for a food containing the vitamin over a food which lacked the vitamin (20, 39). Other experiments have given this same result. Further, individual animals differ in their ability to select foods wisely.

6. Toxic substances are occasionally accepted by animals and men with fatal result. For example, acetate of lead (which is deadly poison) is sweet and definitely acceptable. With other poisons it may be that there is no flavor to the toxic substance or that the flavor of a small but lethal quantity is masked by the flavor of other substances. In any event, it would be difficult to explain the facts of poisoning and of allergy in terms of a need-acceptance theory.

7. Animals and children, even adults, may refuse a medicine which has a foul odor despite the fact that the medicine is known to be curative. This normal behavior places palatability above the requirements of health. The question of bodily need may not enter the picture. Again, people accept and even enjoy foods for which they have an allergy or which subsequently produce ill effects.

8. Feeding habits may regulate the selection of foodstuffs with little regard to bodily needs. A rat may persistently take the food presented in a fixed position or in a favored container regardless of its quality. Such indiscriminate feeding can have no possible relation to nutritional need. Moreover, an established habit of feeding may lead to selection of food which is di-

rectly opposed to known bodily need (33).

Now a candid study of the above facts will convince the impartial reader that the meeting of bodily need is one thing and food acceptance something else. Although food selections often are in accord with nutritional needs, the correlation between need and acceptance is far from perfect. Food acceptance is regulated by the characteristics of the food object (palatability), by the environmental surroundings of the food object, by established feeding habits, as well as by intra-organic chemical conditions which themselves may or may not be directly related to metabolic needs.

In the present paper the writer is basing a theory of food acceptance upon the assumption that contact between head receptors and a food object produces an immediate affective arousal. When a rat tastes and touches and smells a food there is an immediate liking or disliking, an enjoyment or disgust, with a certain degree of affective intensity. To put the matter bluntly: our work leads to the view that rats accept foods which they *like* (find enjoyable) and that foods differ in the degree to which they arouse immediate enjoyment. A preferred food is more intensely enjoyed than one which is not preferred.

This view, that the selection of food-stuffs is determined by the degree of enjoyment, is obviously different from the view that animals accept foods which they *need* to maintain homeostasis and to survive. The two interpretations, however, are not logically opposed, and they may be regarded as supplementary.

Fortunately, the relation between nutritional need and affective arousal can be explored experimentally. Available results show that to an appreciable extent rats *like* foods which they *need*.

In general, foods which are required to maintain homeostasis or to cure deficiency symptoms 'taste good' to the animal. The question should be asked: To what extent do animals accept foods which the nutritionist tells us they need? And what bodily mechanism regulates food acceptance in such a manner that bodily needs tend to be met?

There are other more fundamental questions which are easier to ask than to answer: How can affective arousal be described in terms of brain dynamics and in terms of biochemistry? How are the affective processes related to the formation of specific food-seeking and food-selecting drives? How are affective processes related to the organization of purposive behavior and to learning? Some of these questions will be considered below.

FOOD-SEEKING DRIVES AND AFFECTIVE AROUSAL

General hunger. The prevailing theory of general hunger may be called the *need-stimulus* theory. According to the need-stimulus theory, general hunger is a condition of need produced by the privation of food. The mechanism of the general hunger drive is usually pictured in terms of the contractions of an empty stomach. The empty stomach contracts, thus stimulating nerves within its muscular wall. The frequency and persistence of gastric hunger contractions increase as the general need for food increases. Stimulations from these contractions excite the organism, raising the level of general activity; they also underlie the subjective hunger pang. The condition of hunger is normally relieved by ingestion of food which puts an end to gastric hunger contractions and meets the bodily need for food.

Through a process of conditioning, according to the prevailing theory,

food-seeking behavior becomes increasingly controlled by environmental factors. The usual view is that acts which meet the bodily need and reduce the hunger contractions are 'reinforced.' Primary 'reinforcement' comes from the meeting of a primary bodily need such as the need for food. 'Secondary reinforcement' comes from stimulus-patterns more or less directly associated with the reduction of a primary need. Thus the visual and auditory stimulations from laboratory gadgets which deliver food are said to furnish 'secondary reinforcement.' Through 'secondary reinforcement' the rat learns to run a maze, to make a visual discrimination, to press a bar, to dig through sand, to endure an electric shock, to run on a preference apparatus in order to reach and ingest food.

This development of an increasing environmental control over feeding behavior has been aptly called the *externalization of drive* (1). The doctrine of externalization implies the existence of an original internal drive. The hypothesis of externalization becomes unnecessary, however, if we recognize that all food-seeking drives are learned on the basis of an immediate enjoyment of food.

Internal hunger, of course, does exist. The hungry infant does cry and scream and kick. The nipple in the mouth does bring quiescence. No one wants to close his eyes to any important group of facts.

But the prevailing theory of general hunger drive is inadequate. It has been criticized on a number of counts which will not be reviewed here. We will point out only that this theory has no explanation to offer for specific hungers. Nor does it have an explanation for the facts of food preference.

Specific hungers. The argument for the existence of specific hungers, or ap-

petites, rests upon independent variability in the intake of foodstuffs. In the laboratory the average daily intake of one substance—protein, fat, carbohydrate, water, sodium, phosphorus, calcium, riboflavin, etc.—is found, under certain conditions, to vary independently of the intake of the others.

Independent variability in the intake of foodstuffs can be demonstrated by surgical operation. Removal of the adrenal glands, for example, increases the intake of sodium. Removal of the parathyroids increases the intake of calcium. Changes of relative intake occur during pregnancy and lactation. There are changes with age. And the removal of one substance from the diet is often associated with compensatory changes in the intake of other substances. The interrelationships, as we know them, are complex.

Independent variability in the intake of separate foodstuffs can be shown by experiments upon partial satiation. We have shown that a food preference can be reversed by satiating a rat upon the preferred food. When satiated upon one kind of food, however, the animal continues to accept other kinds of food. An instance of partial satiation is found in some observations which were reported by Bousfield (5) upon the feeding of cats. He reported that when cats are fed to satiation upon one kind of food they continue eating other foods and yield further curves of approach to satiation. Thus by measuring the quantities of food ingested Bousfield was able to demonstrate and determine the extent to which eating one kind of food reduced the strength of drive to eat another.

Independent variability of appetites in the selection of foods has been demonstrated clearly by Richter's method of self-selection feeding. The strength of this method lies in the fact that it presents to the animal a constant and

controlled nutritive environment. The method makes it possible to observe changes of intake for a group of substances and to study the conditions upon which such changes depend. With this method Richter has demonstrated independently varying appetites, or specific hungers.

One difficulty with Richter's method, as used thus far, is that a clear distinction has not been drawn between appetite and palatability or, more simply, between the intraorganic and the environmental determinants of food acceptance. To illustrate this difficulty let us consider Richter's technique for determining the preferential taste threshold. In these threshold determinations the main variable is the concentration of solution. If the organic state is held constant, the preference between distilled water and solutions of sodium chloride is found to vary with the concentration of solution. Since these differences depend upon the characteristics of the food object, one can argue that they are differences in *palatability* rather than differences in *organic appetite*.

The distinction is more basic than the words suggest. Studies of food preference reveal the existence of preferences with almost any pair of test-foods picked at random. The preference tests yield plenty of evidence for independent variability in food selection. But shall we assume from this fact that every pair of test-foods yields evidence for the existence of independently variable *appetites*? Hardly, because in that event we would have to assume almost as many specific appetites as there are different kinds of foodstuff.

The writer believes that to make the concept of appetite, or specific hunger, precise there must be added to the requirement of independent variability one further requirement: *the independent variability in food acceptance must*

be shown to depend upon intra-organic chemical conditions and not merely upon the characteristics of the food-stuff as such. To demonstrate separate appetites, or specific hungers, therefore, one must show independent variability in intake which depends upon intra-organic conditions.

From this point of view Richter's demonstration that the preferential salt threshold is lowered by adrenalectomy is a fact of great importance for the theory of appetite because adrenalectomy does constitute a change of intra-organic conditions.

At the present time the bodily mechanism which regulates selective food acceptance is not known. Richter has assumed that animals become sensitized to substances which are required to maintain homeostasis and hence for existence. This mechanism, if it exists, is one of obvious biological utility. If an animal needed sodium, for example, the preferential threshold for this substance would be lowered.

In a study of the threshold for sodium chloride, Bare (2) confirmed Richter's finding that the preferential threshold is lowered by adrenalectomy, but he drew an important distinction between the *absolute sensory threshold* and the *preferential threshold*. Adrenalectomy lowered the preferential threshold, but it did not change the absolute sensory threshold for sodium chloride as determined by electrophysiological methods. In other words, the difference between normal and adrenalectomized rats is not one of gustatory sensitivity but rather one of preference. This finding again raises the question of the nature of preference and the bodily mechanisms of preferential food selection.

Young (31) pointed out that selecting and balancing a diet according to bodily needs can be explained by a mechanism of selective satiation as

readily as by the lowering of gustatory thresholds. When an animal steadily eats a specific kind of food, the responsiveness of the gustatory cells decreases, we may assume, until at satiation the responsiveness becomes zero. In this connection, it should be pointed out, Richter's method of continuous exposure permits a rat to ingest a food up to the limit of satiation and to keep himself satiated throughout the 24-hour period. It may very well be, of course, that satiation has nothing to do with the gustatory cells as such but that it is a more general physiological phenomenon.

The discovery of the bodily mechanisms of specific hungers, or selective appetites, will have to await the accumulation of further facts. It is our contention that the essential facts must be found within the science of behavior. Accepting one food, rejecting another, accepting with more or less avidity, preferring one food to another, running or working or accepting pain to obtain food, learning to operate the innumerable laboratory gadgets which lead to food—what are these other than *behavioral* facts? Physiologists and nutritionists have not been very enlightening in the matter of specific hunger, palatability, and feeding habit. This is probably because they have lacked the benefit of sound behavioral fact and psychological principle.

The basis of food-seeking and food-selecting behavior. The terms *specific hunger* and *appetite* suggest more than a state of bodily need. They suggest a desire or specific craving which has its basis within the tissues. They suggest that within the organism there are mechanisms which determine the seeking and selecting of foods. What is the nature of the physiological mechanisms which regulate selective food acceptance?

Before attempting to answer the question let us examine a bit of experimental evidence (34):

A rat is placed in the starting-box of our preference apparatus. After 60 seconds the door is opened and the animal is free to move forward upon an open field to sugar or casein exposed in a glass tube 38 cm. in front of the door. After a nibble the food is lowered out of reach. The rat now explores the apparatus and sooner or later returns to the starting-box from which he is removed. The experimenter has measured with a stopwatch the time between release from the starting-box and initial contact with food. In the experiment under consideration there are two groups of rats. The animals in one group are rewarded on each daily run with a nibble of sugar; those in the other group, with casein. At the start all rats spend considerable time exploring the apparatus and some of them seem to find the food quite accidentally. With practice the animals run more and more directly to the food. Occasionally a practiced rat may be seen poised at the door of the starting-box and oriented toward the food (especially with sugar). After a few days of practice there is this difference between the two groups. The animals running to sugar accept this food almost at once; they do not pause to explore until after the food has been lowered out of reach. Their speed of locomotion in approaching the sugar steadily increases from day to day and their day-to-day variance of performance is relatively low. In contrast, the rats running to casein are slower; they delay longer before accepting the food and sometimes pause to explore before accepting it. Their performance shows less change with practice and the day-to-day variance of performance is definitely greater than that of the sugar-incentive rats. Since all experimental conditions are the same for the two

groups, except the kind of food, we may conclude that observed differences in behavior are dependent upon the kind of food offered as a reward or incentive.

The observed difference between running-to-sugar and running-to-casein can accurately be described as a difference in the strength of *behavioral* drive. This difference cannot be explained by reference to metabolic needs since in this particular experiment all rats were well nourished, free from any known metabolic need, and free from general hunger throughout the experiment. The behavioral difference between running-to-sugar and running-to-casein cannot be attributed to practice, since the frequency and distribution of runs were the same for both groups. At all stages of practice the food-approach time was less for sugar-incentive rats than for casein-incentive animals. The difference in behavior between running-to-sugar and running-to-casein cannot be referred directly to gustatory stimulation since the test-foods were out of the range of head receptors during most of the run and since, with only one run per day, there was an intervening period of 24 hours between successive gustatory stimulations. Upon what does the difference in food-seeking behavior depend?

The most probable answer is that with practice the animals developed a neuromuscular set which directed them to the food in the center of the apparatus. This set maintained a persistent orientation toward the goal and its tension component was responsible for release of energy in food-seeking behavior.

We assume that when a practiced rat is placed on the apparatus there is reintegrated a preparatory set. Along with this there is a proprioceptive tension associated with the preparation to run to food. The proprioceptive tension, implying changes in muscle tonus, is a persistent motivation within the

food-oriented rat. Persistent drive-stimulation comes from the muscles, tendons, and perhaps the joints, when a specific food-seeking determination has been activated by the environmental situation. This proprioceptive tension, we assume, is greater in the running-to-sugar drive than in the running-to-casein drive.

When the animal is removed from the apparatus this proprioceptive tension relaxes. Then all that is left of the neuromuscular set is a learned neural organization which remains as a potential basis for reintegrating the set when the rat is again placed in the starting-box. The neural organization, we assume, may persist indefinitely apart from tonic changes in the muscles and independently of the chemical conditions within the body which regulate the relationships of palatability. The acquired neural organization holds over from day to day as the animal gradually learns to run to a particular food.

In the experiment under consideration tests of preference showed that rats prefer sugar to casein. When given repeated choices between these foods they accepted sugar with increasing frequency and casein with decreasing frequency. The acquired running-to-sugar drive acted as a selective factor in the situation.

The behavioral running-to-sugar drive is definitely and consistently stronger than the running-to-casein drive at all levels of practice. Our criteria for determining the strength of drive are: the speed of locomotion, the latency of food acceptance, and the results of direct tests of preference. We would predict that other measures of the strength of drive (of which there are a good many) would give the same result in the comparison of these two food-seeking drives.

If we could fathom the conscious experiences of the rat, we would probably detect a difference between running-to-

sugar and running-to-casein. Certainly a man recognizes a qualitative difference between the expectancy of one kind of food and the expectancy of another kind of food. Moreover, the behavior of the rats suggests that they *like* sugar better than casein, that sugar is more enjoyable than casein. Insofar as rats resemble men, qualitative differences in expectancy and in enjoyment exist.

But apart from speculation there can be no doubt that quantitative differences do exist in the strength of the behavioral food-seeking drives, which differences are dependent upon the *kind* of food, and that the strength of drive is directly related to the palatability level of the foodstuffs.

Affective processes and food-seeking determinations. Our interpretation of the facts is that when the head receptors make contact with a specific food there is an immediate enjoyment with a definite intensity of affective arousal. The behavior of the total organism acts to continue and preserve this enjoyment. It is to preserve enjoyment that neuromuscular determinations are organized. Moreover, the intensity of enjoyment is correlated with the strength of determination to continue and preserve it. For example, the determination to continue eating sugar is stronger than the determination to continue eating casein.

We will tentatively call this view the *affective-determination* theory to have a designation which stands in contrast with the prevailing *need-stimulus* theory of drive. According to the affective-determination theory all specific food-seeking and food-selecting drives are learned. They are organized cortically to continue and preserve an immediate enjoyment of foods which in some degree are acceptable.

According to the need-stimulus theory 'primary reinforcement' comes from

the relief of need. According to the affective-determination theory the relief from distress in any form is an affective change. The organism organizes central determinations which continue those behavioral patterns which lead to the relief of distress. Food-accepting behavior may be organized on the affective basis of relief from organic hunger (as postulated in the need-stimulus theory) but actually the experimental facts point more clearly in a different direction. It is an immediate positive affective arousal (enjoyment of food) which leads to the organization of food-seeking determinations rather than the more remote and delayed relief of gastric hunger. Food deprivation operates in some way to make the foods of which the animal is deprived more enjoyable when contact with them has been made.

According to the need-stimulus theory 'secondary reinforcement' comes from the activities of chewing and swallowing food and from the environmental stimulations which immediately precede these activities. These activities become 'reinforcing' because they are associated with the reduction of gastric hunger. According to the affective-determination theory it is the immediate and direct contact of head receptors with foodstuff which produces an affective arousal and on the basis of this affective arousal a food-seeking determination is organized.

Food-seeking drives and food selection may also be based upon the relief from internal distress, especially from the symptoms produced by deprivation of a needed substance. In a pioneer study of the appetite for vitamin B, Harris *et al.* (9) have shown that the rat must *learn* to associate relief from deficiency symptoms with some characteristic of the food before the appetite for the vitamin can be demonstrated. More recently, Scott and Verney (17)

have confirmed this view. They concluded that the appetites for thiamine, riboflavin, and pyridoxine are acquired on the basis of a subjective feeling of well-being which is associated with some characteristic of the vitamin-containing food. Again, after an extended series of experiments with chemically pure foods, Scott and Verney (18) concluded (in agreement with the present writer) that the choice of foods is not directly dependent upon their nutritional nature but rather upon the animal's subjective response.

According to the need-stimulus theory of drive the source of motivation is found in persistent stimulations from tissues in need, such as the contractions of an empty stomach or the persistent pain-pressure stimulations from the parched throat in thirst. According to the affective-determination theory persistent motivation comes from the proprioceptors when the organism is in a set with expectant tension.

When a rat is placed in an environmental situation (apparatus) to which he is well habituated there is redintegrated in him a specific neuromuscular determination. This redintegration builds up a more or less persistent proprioceptive stimulation which furnishes physiological drive. But wholly apart from proprioceptive stimulation and preparatory adjustment the well-trained animal responds to environmental stimulations, immediately and automatically, with patterns of learned behavior which lead him to food. The food-seeking patterns may become so completely automatic that they resemble reflexes.

According to the need-stimulus theory the strength of drive varies with the degree of need. But it has been shown experimentally that the relation between the strength of the behavioral hunger drive and the period of food deprivation is not linear. The strength of behavioral drive reaches a peak after several

days of deprivation and then declines; but the need for food increases steadily with deprivation until death through starvation. Despite this difficulty one commonly reads of a 24-hour hunger drive or a 48-hour drive! One reason why the quantitative psychology of food-seeking motivation has been so backward and inexact lies in the fact that the strength of hunger and thirst has been controlled solely through deprivation, palatability relations being ignored.

According to the affective-determination theory the strength of behavioral drive, as actually observed and measured, varies with many conditions (36). There are three main groups of parameters: (1) Intra-organic conditions including specific hungers and non-appetitive constitutional factors. (2) Environmental conditions including palatability and non-palatability factors. (3) Feeding habits and attitudes already acquired by the organism. To control the strength of drive one should keep *all* of the factors constant except one which is experimentally varied. For example, the strength of drive might be controlled by varying the palatability level of the incentive food. One might vary only the concentration of saccharin solution presented as a reward to well-nourished, non-hungry rats (3, 10).

Any theory of food-seeking and food-selecting drive must explain the facts. The affective-determination theory offers a plausible explanation of facts brought to light in experiments upon food acceptance: the uniformity and stability and consistency of food preferences, the transitive series or hierarchies of preference which have been repeatedly found, the dependence of food acceptance upon the characteristics of the food object as well as upon the chemical state of the organism, the correlation between the observed strength

of behavioral drive and the measured level of palatability, the gross fact that adequate motivation can be obtained with a food incentive in the absence of bodily need, and similar facts. The need-stimulus theory simply does not offer satisfactory explanation for any of these facts.

Some related work. Bindra (4) believes that the motivation for hoarding food is the same as that for eating food and that a non-alimentary mechanism determines both hoarding and eating. Interestingly enough, he found that the number of units of food hoarded was greater when a wet mash containing saccharin was offered than when pellets of Purina Chow were presented. The sweet wet mash was preferred in hoarding even though the saccharin has no utility in meeting bodily need! It is of theoretical importance that the more palatable food evoked the greater activity of hoarding. This is in line with our repeated finding that the more palatable food evokes the greater rate of running.

Bindra's reference to a non-alimentary mechanism raises an interesting problem. A neuromuscular set with proprioceptive tension is a non-alimentary mechanism. It is likely, however, that the bodily preparation for a particular kind of food includes changes in the tonus of the smooth muscles of the alimentary tract and changes in the activity of salivary and gastric glands. It is the organism as a whole which is prepared for food. We would predict the existence of quantitative relations between the degree of salivary secretion and the palatability level of the food incentive. The relation between the alimentary and non-alimentary factors in food-seeking drive is something which needs to be studied experimentally.

Another paper, pertinent to our present discussion, is that of Elliott and Bousfield (7). After reviewing the facts

relative to deprivation these writers have pointed out that it is difficult to demonstrate any simple and general relation between deprivation and behavior. The various drives have similar behavioral effects and probably have similar bodily mechanisms.

In certain respects, Elliott and Bousfield argue, drives are like emotions. In both there are two basic mechanisms of motivation. First, there is a proprioceptive mechanism which builds up muscular tensions. In so far as can be determined from the available data, the effects of proprioceptive tension and hunger contractions are similar. In both there are volleys of afferent nerve impulses which result in the facilitation of skeletal reflexes and of central processes. One and the same mechanism may be assumed for proprioceptive tension and hunger contractions. Second, there is a sympathico-adrenal mechanism which operates in hunger and in other emergency conditions. These two mechanisms, the proprioceptive and the sympathico-adrenal, account satisfactorily, Elliott and Bousfield believe, for the varied behavioral effects of hunger.

We agree with their analysis so far as it goes. Our research to date, however, has tended to emphasize the first of these mechanisms—the proprioceptive mechanism. We believe that the selection of food is fundamentally a biochemical process and that its bodily mechanism is more general and more complex than is the sympathico-adrenal mechanism alone.

Relative to the proprioceptive mechanism a brief methodological paper by Geier (8) is of interest. Geier described a technique for measuring the bodily tension associated with expectation of food. He placed rats for one minute prior to feeding (or to non-feeding) in an activity wheel. By recording their activity he demonstrated that rats 'expecting' food made more revolutions per

minute than similar rats not 'expecting' food. Underlying this work is the assumption that when a rat is 'expecting' food a tension of expectancy is built up. The rat can reduce or work off this tension by running in an activity wheel. Geier's work is based upon Tolman's well-known hypothesis of expectation. Assuming the method to be sound, we would predict that rats 'expecting' a highly palatable food such as sucrose would be more active in the wheel than those 'expecting' a food of low palatability such as casein. An experimental test of this hypothesis could readily be made.

In concluding this section we point out that the above interpretation of food-seeking and food-selecting behavior recognizes the fundamental importance of affective arousal in the organization of drives. The theory is frankly hedonic. Affective arousal in the rat is assumed on the basis of the rats' behavior and human experience. Some day, we believe, affective arousal will be described objectively in terms of brain dynamics and the underlying biochemical processes. For the present the assumption of affective processes within the rat appears to be the simplest hypothesis for interpreting the available data upon relative food acceptance.

The assumption of affective processes has wider implications than those within the area of food acceptance. The assumption of affective arousal is fundamental in such broad fields of investigation as the study of pain avoidance, anxiety, neurosis, sexual behavior, as well as in the general theory of motivation.

The above analysis brings together three fundamental aspects of motivation which have heretofore been treated somewhat apart. First, psychological hedonism, an ancient doctrine, is implied by the assumption that affective processes organize determinations which

are expressed in behavior and that there is a quantitative relation between observed strength of drive and affective intensity. Second, the theory of organic set or determination has long been central in the analysis of motivation. In the present study we have assumed that determinations (organic sets) are organized to continue enjoyment and to relieve distress. Third, persistent organic stimulation has long been recognized as a factor in physiological drives. The present emphasis upon proprioceptive tension indicates a source of persistent drive stimulation in lieu of (or in addition to) the usual accounts of stimulations from tissues in need.

These three aspects of motivation— affective process, neuromuscular and neural determination (set), proprioceptive stimulation or tension—are, therefore, related to each other in the present theoretical analysis. Further, we have emphasized the fundamental importance of biochemical factors in the determination of relative food acceptance.

AFFECTIVE AROUSAL AND LEARNING

In current discussions of learning the word *reinforcement* frequently occurs. What does it mean? The writer, for one, does not know. The word suggests a strengthening similar to that due to practice, or training, but clearly the word does not refer to the effects of exercise as such.

The ingestion of food is said to 'reinforce' patterns of behavior which lead up to ingestion. In a recent paper presented to the American Psychological Association, the quantity of food consumed is accepted as an index of the quantity of 'reinforcement.' Here is reinforcement measured right down to the fraction of a gram! The relief of general hunger is commonly described as 'reinforcing.' The relief from anx-

iety, according to O. H. Mowrer, is 'reinforcing' since patterns of behavior which reduce anxiety are repeated and learned. What is reinforcement?

The word *reinforcement* appears to carry the implication that affective processes are importantly related to learning. But how are affective processes related to learning? The answer is not clear from current discussions. Obviously, certain stimulus-objects do not reinforce. Perhaps they are affectively indifferent (?). Other stimulus-objects relieve distress and this relief 'reinforces' behavior. Positive enjoyment, however, as well as relief from distress, appears to provide 'reinforcement.'

If we look at this matter historically, we find that for a good many centuries the pleasure-pain theory of learning was quite generally accepted. Affective processes were frankly recognized as being related to learning. In modern times Thorndike formulated his law of effect; affective processes were admitted in the form of satisfaction and annoyance. Under criticism Thorndike abandoned affective processes, largely because of their subjective nature, and stated the law of effect objectively in terms of behavior. Like the family cat, however, who was kicked out the back door and later slipped in at the front door, affective processes were ejected (or should we say evicted?) from American psychology. Soon it became respectable to talk about *reinforcement*. Have we been duped?

In considering the problem of affective processes as related to learning we will define learning as a modification of behavior and of neural organization which is produced through practice, or training. The making of an organized response is the *sine qua non* of learning. An animal must act to learn and he learns precisely the activity exercised. With practice, or training, an acquired pattern of response becomes

smoother, better organized, more readily elicited.

Now affective processes are not necessary for learning to occur. Sheer repetition when the affective state is indifferent may result in learning. Moreover, consciousness is not necessary for learning to occur. A decorticate dog (presumably unconscious) is capable of learning. Even the simultaneous excitation of two neurons may result in a functional change which can significantly be described as learning.

Since affective processes are not essential for learning, just how are they related to learning? In terms of brain dynamics we think of enjoyment as associated with the process of organizing patterns of response and with the maintaining of recently organized response patterns. We think of distress as related to a central disorganization produced through frustration, intense stimulation, and certain sensory excitations such as those from bitter substances. The relief from distress is related to the organizing and maintaining of patterns of response which produce relief.

Thus enjoyment, distress, and relief are intimately associated with the organizing and disorganizing of patterns of response. Is the *process of organizing* to be regarded as a form of learning? We think not. If *learning* is defined as the *process of organizing*, the definition of learning becomes so broad that it is practically useless in psychology. It is better to think of learning in terms of the fixation of organized response patterns through exercise. A single response may result in some learning, but practice and training imply the repetition of an organized response.

Of course, there are conditions other than affective arousal which lead to psychological organization and to disorganization. Gestalt psychologists have long stressed the fact that perceptual organization is determined by the configura-

tion of energy within the stimulus field as well as by the physical structure of the brain. Again, habits and attitudes which have already been learned are themselves factors in further psychological organization. Practice itself makes for a smoother, better integrated response, and is thus a factor in the development of psychological organization.

Enjoyment and the relief from distress, therefore, are conceived as intimately related to psychological organization and disorganization.

The relation between practice and affective arousal. If rats are given a series of runs on our apparatus for a single food incentive, their rate of running increases steadily with practice. For this reason the *absolute* rate of running is not a dependable index of the palatability level of the test-food. The *relative* rates of running for different foods, however, agree with the palatability ratings when practice is the same for all. If the frequency and distribution of runs are the same, rats at all levels of practice run consistently faster to a preferred food than to one which is not preferred. The results suggest that practice and palatability level are independent factors which determine the rate of running to food.

Several of our experiments throw light upon the relation between practice and palatability:

In one experiment an attempt was made to reverse the preference of sugar to casein by training the rats to run for casein alone, without choice. The general plan of the experiment was to give a series of brief tests of preference between sugar and casein. Practice in running for casein, the non-preferred food, was interspersed between these tests. The results showed clearly that the rate of running for casein increased steadily with practice but that practice

up to 1000 runs per rat did not disturb the initial preference for sugar. On the contrary, the percentage of preference for sugar increased steadily from test to test (despite the running for casein) until at the close of the experiment there was a 100 per cent preference for sugar. A control group without intervening practice also exhibited a consistent preferential trend toward a 100 per cent preference of sugar to casein (32).

In an earlier experiment under somewhat different conditions the opposite result had been obtained. The rats of one group had been trained in running to wheat and those of another group had been trained in running to sugar prior to any test of preference. When the preference between wheat and sugar was first tested the wheat-habituated rats continued to select wheat and the sugar-habituated rats continued to select sugar. During repeated tests the preference for wheat was stable but the preference for sugar weakened and with some animals it reversed. The results suggested that the particular diet of the experiment would support a stable wheat preference but not a stable sugar preference (28).

These two opposed results can be reconciled by recognizing that sugar and wheat are nearly equal in palatability and that casein is distinctly less palatable than either sugar or wheat (under dietary conditions of the experiment). If test-foods are nearly equal in palatability, practice may be temporarily effective in determining choice; but if the test-foods differ widely in the level of palatability, practice in running for the less palatable food is ineffective in changing a preferential selection. Practice may develop a food-accepting habit but palatability rests upon a chemical basis and is something which is independent of practice.

Other experiments with the preference technique leave no doubt concerning the

great importance of habit as a determiner of food selection.

In the introductory paragraphs of this paper we pointed out that animals *learn* a preferential discrimination and that the percentage of preference increases with practice. In early experiments we used the technique of substituting one pair of test-foods for another or of abruptly changing one member of the pair. This technique revealed abrupt changes in the percentage of preference followed by gradual preferential trends. In one such experiment, for example, tests of preference were made with sugar and three other test-foods; sugar was consistently preferred to them all. Then milk (a new food) was tested with sugar. At the start the animals continued to select sugar but with continued testing the preference weakened and then reversed. The last tests revealed an unmistakable preference of milk to sugar. The curve of preferential trend was in the form of a gradual wave first above and then below the 50 per cent sugar-acceptance line. This curve of preferential trend can be explained by assuming that the rats were habituated to selecting sugar, a highly palatable food, before milk was introduced. When milk, the most palatable food of all, was presented the sugar-selecting habit gradually weakened and the milk-selecting habit gradually strengthened (23).

We noted above that a preference can be reversed either by satiating rats upon the preferred food or by depriving them of the non-preferred food. But in neither case does the reversal occur at once. On the contrary, there are gradual changes in the relative frequency of selecting the two foods until finally, with practice, the original preference is reversed. The gradualness of these preferential changes indicates that a percentage of preference is dependent upon practice and that for this reason

a fixed percentage of preference cannot be used as a univocal index of a difference in palatability.

In the experiment of Young and Chaplin (37) the continued preference of sugar to casein, despite a marked bodily need for casein, was finally explained in terms of habit. The interpretation affirms that an established preferential habit can regulate the selection of food independently of bodily need and even contrary to need and to normal palatability relations. When the rats in this experiment were forced to form a new preferential habit on a new kind of apparatus they at once selected casein in preference to sugar. The new preferential habit revealed the true palatability relations under the changed dietary and testing conditions. An established feeding habit, therefore, may dominate the choice of food irrespective of bodily need and irrespective of the chemically-determined and stable relationships of palatability.

Perhaps this result can be regarded as an instance of the functional autonomy of habits—the persistence of habits despite change in the underlying conditions of need. Further examples of the dominance of habit mechanisms despite changes of internal organic state are to be found in the experiments of Spence and Lippitt (19) and Brogden (6) which will be discussed below.

The total evidence indicates clearly that a habit mechanism may regulate the preferential selection of food independently of palatability. A habit mechanism may even obscure the true relationships of palatability and for this reason practice must be carefully controlled before making any generalization about palatability. Practice is one thing; affective arousal is something else.

Practice and affective intensity as independent determinants of the strength

of behavioral drive. Experiments upon food acceptance have shown clearly that the strength of behavioral drive in running to food depends upon at least two independently variable conditions. First, practice is an important determinant of the rate of running to food. It is beyond question that the strength of an acquired food-seeking drive varies directly with the frequency and temporal distribution of runs. Second, there is a direct quantitative relation between the intensity of affective arousal (as shown by tests of preference) and the strength of behavioral drive toward a specific food (as measured by the rate of running and the latency of food acceptance) (33, 34).

In other words, the measured strength of behavioral drive varies both with practice and with affective intensity. These two determinants of the strength of drive are relatively independent of each other. Evidence that practice and affective intensity are independent determinants of the strength of drive can be summed up in the following three points:

1. It has been repeatedly observed that practice brings an increase in the percentage of choices of the preferred food. Practice raises the percentage of choices to a ceiling somewhere between 50 and 100 per cent. But practice does not change the palatability relations among a group of test-foods. In one experiment three test-foods were used and the three pairs of foods were given equal practice. All the percentages of preference changed with practice but the hierarchy of relative palatability remained the same at every stage of practice (33). Practice revealed the preferential relations with increasing clarity and definiteness, but practice did not change the chemically-determined relationships of palatability. Practice and palatability are distinct and independ-

ent determinants of the percentage of preference.

2. When rats are run on our apparatus for a single food, without choice, the rate of running steadily increases with practice. If the daily runs are omitted for a time (as during a vacation period) the rate of running is lower after the pause. There can be no doubt that the rate of running for a single food varies with the frequency, the recency, and the temporal distribution of runs. Now if the two foods which differ markedly in palatability are employed as incentives, and the frequency and distribution of runs are the same for both, the rats run consistently faster for the preferred food. At all stages of practice the preferred food evokes the higher rate of running. In other words, to repeat, the rate of running for a single food is dependent upon two independently variable factors: the level of palatability (intensity of affective arousal) and the number and temporal distribution of runs (33).

3. In one experiment rats were given a single run per day at the same hour of day to keep the factor of practice constant. Under these conditions it was found that rats running to sugar ran consistently faster *at all stages of practice* than those running to casein. These incentive differences in the rate of running cannot be attributed to practice since the factor of practice was constant. The differences depend upon the quality of the reward and they are motivational in nature.

To equalize motivational differences we added together, cumulatively, the total daily running times for each rat regardless of the kind of reward, and then we called the total time for each rat unity. This procedure equalized motivational differences due to the kind of food and to constitutional factors which are reflected in the speed of locomotion. The total cumulative ap-

proach time for each animal was then divided into thirds. The number of runs made by a rat during the first third, the second third, and the third third of his total cumulative time was determined from the data sheets. The group data were plotted. The curves showed an increase in the number of runs with practice from one third to the next third of the running time. But under these conditions the practice function was the same for all three test-foods. The rats did not *learn* faster for one food than for another. We concluded that *learning* is dependent upon the number and distribution of runs and that when practice is held constant the *rate of learning* does not vary with the palatability of the test-food. The *rate of running*, however, is directly related to palatability and this relation appeared clearly at all stages of practice. The rate of running is dependent upon both motivation and practice which are independent variables (33, 34).

This conclusion may appear not to agree with common sense expectation based upon a confusion of performance (which depends upon many factors including exercise) and learning (which depends upon exercise alone). The teacher knows that Johnnie works his arithmetic better for a good reward than for a bad one. Of course, if our rats had been run continuously for 15 minutes daily, the sugar-incentive rats would have made more runs during the period than casein-incentive animals. For this reason alone they would have *learned* faster with a sugar incentive. But in a fair comparison of incentives the number and temporal distribution of runs must be held constant and only the one factor in which we are interested (palatability) varied.

The conclusion can be stated as a paradox: The rat learns to *run faster* to sugar than to casein, but he does not

learn faster to run to one food than to another.

The conclusion to which we are drawn has a clear bearing upon the much discussed law of effect (14). It is our view that the laws of learning are entirely laws of exercise, practice or training, and that affective arousal is only secondarily related to learning. Affective arousal is related primarily to the organizing and disorganizing of psychological processes. If an organized response is made, to prolong enjoyment or relieve distress, this organized response leaves after it some neural trace. This neural trace is fixated by exercise rather than by affective processes.

The above lines of evidence and argument lead to a single conclusion: The strength of behavioral drive in food-seeking depends upon two relatively independent factors—exercise and the intensity of affective arousal.

The acquisition and reintegration of food-seeking determinations. A positive or pleasant affective arousal results in an organized response to continue or preserve the enjoyment. A negative or unpleasant affective arousal results in various signs of psychological disorganization. The relief from distress is an affective process which organizes behavior to continue or preserve relief.

Any response which an organism makes may leave after it some physical change within the nervous system which is the physical basis of learning.

Positive affective processes lead to the organization of neural determinations which are shown in persistent purposive behavior. An intense enjoyment organizes a strong drive; a less intense enjoyment organizes a less strong drive. In every day terms we might say that there is an identity between degree of enjoyment and degree of desire.

When positive affective arousal is repeated day after day the organizing

process is repeated. At this point learning comes into the picture. When a naïve rat is placed in our apparatus he explores timidly and finds the food reward seemingly by accident. When a practiced rat is placed in the apparatus and later released from the starting-box he runs promptly and smoothly to the food. Clearly he has learned something. With the practiced animal the stimulus-patterns from the apparatus redintegrate an acquired determination to run to food. Possibly there is an expectancy of a particular kind of food. The expectant set is shown by occasional attempts to raise the door prematurely and by the bodily orientation toward the door and the food. But even when these activities do not occur the animal still runs promptly to the food when the door is opened. Visual and auditory stimulations from the apparatus presumably release the acquired purposive behavior.

The rat in the starting-box awaiting release resembles an automobile driver poised before a traffic light. Flashing of the green light releases a pattern of behavior for which the subject is already prepared.

There can be little doubt that environmental stimulus-patterns redintegrate food-seeking determinations in the rat. A question of considerable theoretical importance, however, is the one considered in the next section.

Can the organic state redintegrate appropriate food-seeking behavior? Does the hungry animal seek food and the thirsty animal seek water in one and the same environment? To what extent do bodily needs act selectively in building up appropriate food-seeking determinations?

The early experiments of Hull and Leeper demonstrate that rats can learn to take one path to food when hungry and another path to water when thirsty.

Hull (11) argued that the explanation of this discrimination was in the difference between the organic stimulus-patterns of hunger and thirst. Through conditioning, he believed, the animal learned to take one turn in the presence of an environment plus hunger and another turn in the presence of the same environment plus thirst.

Kendler (12) has criticized Hull's interpretation. Kendler deprived rats of solid food and water so that at the time of training they were both hungry and thirsty. He trained these animals in a simple T-maze with water in one goal box and food in the other. The experimental procedure was planned so that all rats would have equal opportunity to explore both goal boxes. In the critical test series the rats were made either hungry or thirsty but not both hungry and thirsty.

Kendler found that the hungry rats were able to go directly to food and the thirsty rats to water. In view of their known organic state their choice of pathway was clearly appropriate. Kendler argued that since the training had been under a single physiological drive (that produced by inner stimulation from simultaneous hunger and thirst), the discrimination of pathway could not be referred to some difference between the organic stimulus-patterns arising either from hunger or from thirst. Consequently, Kendler concluded, Hull's explanation of the hunger-thirst discrimination is not correct.

Our interpretation of the facts would be as follows: During the training period the hungry-thirsty rats enjoyed nibbles of food in one place and sips of water in another place. On the basis of an immediate affective arousal two independent food-going determinations were organized and they were both equally practiced. One determination directed the animal to solid food, the other determination to water. At the

time of the critical test there was clearly some selective factor. What is it? This factor, we believe, is the state of satiation produced by the experimenter. The hungry rats were also water-satiated; the thirsty rats were also food-satiated. Whatever satiation may ultimately turn out to be, it is obviously an organic condition which inhibits the acceptance of a particular kind of nutrient. Hence, since the water-going and food-going determinations were equally practiced at the time of the test, the state of satiation would act and did act selectively by weakening the inappropriate determination. Consequently the hungry rats were univocally determined to go to food and the thirsty animals to water.

Another experiment which is apropos is that of Spence and Lippitt (19). These investigators trained rats to run a Y-maze under thirst motivation. One path led to water. The other path led to food (for half of the rats) or to an empty box (for the other half). Inasmuch as the rats were food-satiated, they did not eat when they discovered the food in the food box. In the critical test series the internal motivation was changed. The rats were now made hungry but satiated upon water.

Spence and Lippitt found that on the first trial the hungry animals inappropriately ran down the water alley to water. In further runs the hungry rats learned to run to food but they learned no faster than the animals which had never found food there during the training period. Mere 'knowledge' based upon sensory inspection of the food but without ingestion was of no avail in re-learning to run to food instead of to water.

Our interpretation of the Spence-Lippitt result is as follows: During training there was no hunger, no food acceptance, no affective arousal through contact with food, and hence no ac-

quired determination to run to food. There must have been some perceptual organization relative to food ('knowledge') but a neural set to run to food was not organized. On the other hand, through enjoyment of sips of water there was definitely organized in all rats a water-seeking determination. This determination was fixated through exercise. When the organic state was changed from thirst to hunger and the hungry animals were placed in the familiar apparatus the environmental stimulus-patterns redintegrated the only purposive determination which had been learned—that which was made manifest by running to water. With further trials the hungry rats organized and practiced a food-seeking determination. The positive affective arousal through contact with food was necessary for this purposive determination to develop.

As to the possibility of an effective 'knowledge' factor, it is our opinion that the Spence-Lippitt experiment does not disprove Professor Tolman's hypothesis. In a fair test there should be equivalent 'knowledge' of the food location and of the water location prior to the development of any selective motivation. This condition was not met by the Spence-Lippitt experiment.

In the present connection an experiment by Brogden (6) has theoretical interest. Brogden found that the rate of extinction of a conditioned leg flexion was nearly the same with hungry and satiated dogs. He demonstrated that stimulations from the apparatus which delivered the food were more important in preserving a learned response than the organic state of hunger or satiety. Brogden interpreted his findings in terms of Anderson's theory of externalization of drive.

Our interpretation would be somewhat different. The leg flexion under the conditions of Brogden's experiment was in the first place organized under

affective conditions—relief from pain and later enjoyment of food. With practice this response became automatic and controlled by environmental conditions. The response became a habit regulated by an acquired neural mechanism. Under these conditions a change from hunger to satiety should not be expected to make very much difference in the extinction time of the conditioned response. A problem arises only when we assume that the original motivation for food acceptance was the gastric hunger tension.

In concluding the present section of our discussion the writer expresses the view that positive or pleasant affective processes lead immediately to the organization of central neural determinations which are expressed in purposive behavior. Behavior moves to sustain and preserve enjoyment as well as to relieve distress. Affective processes are the *raison d'être* of purposive activity. By its very nature enjoyment is something to be maintained or regained; distress is something to be relieved or avoided. The organization of behavior to preserve enjoyment and to relieve distress implies a corresponding neural organization of response patterns. Quite frankly we believe that there is an hedonic steering, directing, principle.

We do not claim that affective arousal is the *sine qua non* of learning; nor that affective arousal 'reinforces' certain behavioral patterns. We claim rather that patterns of purposive behavior are *organized* to preserve enjoyment and to relieve distress, and that enjoyment and relief are *organizing* processes. Further, we recognize other principles of organization and disorganization than the hedonic principle.

GENERAL CONCLUSIONS

The present paper is a study of the theoretical implications of the writer's experiments upon food acceptance and

the relation of this work to current views concerning food-seeking drive, affective process, and learning. On the basis of the discussion the following general conclusions are drawn:

1. Affective processes exist in the rat as truly as in man. When the head receptors, especially those of taste and touch and smell, come in contact with a food there is an affective arousal which we have designated as enjoyment. Different intensities or degrees of enjoyment are revealed directly by the feeding behavior of rats and by tests of preference. Distress produced by deprivation and the relief of distress through food ingestion are also affective processes which are importantly related to food acceptance.

2. An hedonic theory of drive is proposed. Specific food-seeking determinations are organized within the nervous system to preserve the enjoyment of foods and to relieve organic distress produced by dietary deprivation. In general, rats develop drives to run to foods which they *like* (find enjoyable) rather than to foods which they *need* (require nutritionally). There is, however, a positive correlation between what rats *like* and what they *need* but not a one to one relation.

3. The strength of drive in running to a food varies directly with the degree of enjoyment (intensity of affective arousal) of that food. Rats run faster in approaching a highly palatable food and accept it more promptly than in approaching a less palatable food. Consistent differences of palatability are apparent at all levels of practice.

4. Affective arousal, conceived in terms of brain dynamics and resting upon biochemical conditions, is intimately related to the organizing or disorganizing of psychological processes. Behavioral patterns are organized which continue and preserve enjoyment and relieve distress.

5. Learning is defined as a modification of behavior and of neural organization which depends upon exercise, particularly upon practice or training. The making of an organized response, regardless of how it became organized and regardless of its motivation, results in learning.

6. The strength of a food-seeking drive, as measured by the rate of running to food and the latency of food acceptance and by preferential food selection, depends upon two independent variables: the intensity of affective arousal by food contacts, and the number and temporal distribution of runs to food.

These same two factors— affective intensity and practice— regulate the preferential food selections of the rat.

7. The intensity of affective arousal is correlated directly with the strength of food-seeking drive but not at all with the rate of learning. If the frequency and distribution of runs are held constant, rats do not *learn* faster to run to one kind of food than to another. At all stages of practice, however, rats *run* faster to the more palatable food and they accept it more promptly than the less palatable food. This means that affectivity is primarily related to motivation and secondarily to learning.

8. Specific food-seeking and food-selecting determinations are developed through practice. When a trained rat is placed in a familiar apparatus the environmental stimulus-patterns re-integrate an organic set of preparation and expectancy with alimentary and proprioceptive components. There is a proprioceptive tension which, we assume, varies in degree with the strength of drive. Moreover, rats act as if they were expecting a particular kind of food and not just food in general.

9. Relative food acceptance can be investigated as a part of an objective psychology of the affective processes

with a minimum of hypothesis. The parameters of food acceptance form three main groups: intra-organic conditions, environmental (palatability and non-palatability) conditions, feeding habits and attitudes. The theoretical views expressed above rest squarely upon the analysis of laboratory findings and a test of their adequacy can be made in further investigations.

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1903	10	1	2	3	-	-	\$1.00	\$3.00
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1912	19	1	2	3	4	5	\$1.00	\$5.50
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1921	28	-	2	3	4	-	\$1.00	\$4.00
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1923	30	1	2	3	4	5	\$1.00	\$5.50
1924	31	1	2	3	4	5	\$1.00	\$5.50
1925	32	-	2	3	-	5	\$1.00	\$3.00
1926	33	1	2	3	4	5	\$1.00	\$5.50
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1928	35	1	2	3	4	5	\$1.00	\$5.50
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1931	38	1	2	3	4	5	\$1.00	\$5.50
1932	39	1	2	3	4	5	\$1.00	\$5.50
1933	40	1	2	3	4	5	\$1.00	\$5.50
1934	41	1	2	3	4	5	\$1.00	\$5.50
1935	42	1	2	3	4	5	\$1.00	\$5.50
1936	43	1	2	3	4	5	\$1.00	\$5.50
1937	44	1	2	3	4	5	\$1.00	\$5.50
1938	45	1	2	3	4	5	\$1.00	\$5.50
1939	46	1	2	3	4	5	\$1.00	\$5.50
1940	47	1	2	3	4	5	\$1.00	\$5.50
1941	48	1	2	3	4	5	\$1.00	\$5.50
1942	49	1	2	3	4	5	\$1.00	\$5.50
1943	50	1	2	3	4	5	\$1.00	\$5.50
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